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VOL. XVII.

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TRANSACTIONS

OF THE

OBSTETRICAL SOCIETY

OF

LONDON.

VOL. XVII.

FOR THE YEAR 1875.

WITH A LIST OF OFFICERS, FELLOWS, ETC.

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1876.

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Bartrum, John S., Esq., F.R.C.S.	Bath.
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Swayne, Joseph Griffiths, M.D.	Bristol.
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BRITISH SUBJECTS.

Elected

1862 CHURCHILL, FLEETWOOD, M.D., Professor of Midwifery, King's and Queen's College of Physicians, Ireland; Ardtrea Rectory, Stewart's Town, late of 15, Stephen's green north, Dublin. [3, Harcourt street, Dublin.]

1862 MCCLINTOCK, ALFRED H., M.D., late Master of the Lying-in Hospital; 21, Merrion square north, Dublin.

1862 DUNCAN, JAMES MATTHEWS, M.D., A.M., F.R.S. Ed., Lecturer on Midwifery and Diseases of Women and Children, Surgeon's Hall; 30, Charlotte square, Edinburgh.

1870 FARRE, ARTHUR, M.D., F.R.S. (HON. PRES.), Physician Accoucheur to H.R.H. the Princess of Wales; 12, Hertford street, Mayfair.

1871 KEILLER, ALEXANDER, M.D., F.R.S. Ed., Physician to the Royal Maternity Hospital, Lecturer on Midwifery and Diseases of Women and Children at the Surgical Hall, Edinburgh.

Elected

1871 KIDD, GEORGE H., M.D., F.R.C.S.I., Obstetrical Surgeon to the Coombe Lying-in Hospital, and Examiner in Midwifery at the Queen's University and Royal College of Surgeons of Ireland; 30, Merrion square south, Dublin.

1871 TRACY, RICHARD T., M.D., Lecturer on Obstetrics at the University of Melbourne, Physician to the Lying-in Hospital, Melbourne, Victoria. [Deceased.]

1870 WEST, CHARLES, M.D., F.R.C.P., Physician to the Hospital for Sick Children; 61, Wimpole street, Cavendish square.

FOREIGN SUBJECTS.

1872 BARKER, FORDYCE, M.D., Professor of Clinical Midwifery and Diseases of Women at the Bellevue Hospital Medical College, and Obstetric Physician to the Bellevue Hospital; Consulting Physician to the New York State Woman's Hospital, &c.; 85, Madison avenue, New York.

1863 BRAUN, CARL, M.D., Professor of Midwifery, Vienna.

1862 CHANNING, WALTER, M.D., late Professor of Midwifery in the University of Cambridge, 17, Blacon-street, Boston, U.S.

1875 COURTY, AMEDÉE, M.D., Clinical Professor at the Faculty of Medicine of Montpellier.

1863 DEPAUL, JEAN ANNE HENRI, M.D., Professor of Clinical Midwifery, Paris.

1863 FAYE, F. C., M.D., Professor of Midwifery in the University of Christiania.

1864 HECKER, C., M.D., Munich.

1866 HUGENBERGER, THEODOR, M.D., à la Maternité et aux Enfants Trouvés Hôpital des Accouchements, Moscow.

1866 LAZAREWITCH, J., M.D., Kharkoff, Russia.

1863 MARTIN, EDUARD, M.D., Director of the Institute for Clinical Midwifery at the University, Berlin (deceased).

Elected

1864 Pajot, Ch., M.D., Professor of Midwifery to the Faculty of Medicine, Paris.

1866 Rizzoli, Francesco, M.D., Bologna.

1862 Scanzoni, F. W. von, M.D., Professor of Midwifery, Würzburg.

1875 Simon, Gustav, M.D., Professor of Surgery at Heidelberg.

1864 Sims, J. Marion, M.D., late Surgeon to the Women's Hospital, New York.

1872 Spiegelberg, Otto, M.D., Professor of Clinical Midwifery, and Director of the Gynæcological Clinique in Breslau.

1866 Thomas, Abraham Everard Simon, M.D., Leyden.

1872 Thomas, T. Gaillard, M.D., Professor of Obstetrics in the College of Physicians and Surgeons, New York.

1862 Virchow, Rudolf, M.D., Professor of Pathological Anatomy in the University of Berlin.

CORRESPONDING FELLOW.

1873 Martin, A. E., M.D., Berlin.

ORDINARY FELLOWS.

FEBRUARY, 1876.

Those marked thus (*) have paid the Composition Fee in lieu of further annual subscriptions.

The letters O.F. are prefixed to the names of the "Original Fellows" of the Society.

Elected

1874 ADAM, A. MERCER, M.D., Bargate Lodge, Boston, Lincolnshire.

1869 ADAMS, THOMAS RUTHERFORD, M.D., Surgeon to the Croydon General Hospital; Stamford House, St. James's road, West Croydon, S.

1859 ALDERSEY, William Hugh, M.B. Lond., F.R.C.S., South Hayling Island, Havant, Hants.

1871 ALDERSON, FREDERICK H., Southerton House, Dartmouth Road, Hammersmith.

1861 ALDRIDGE, JOHN PETTY, M.D., Shirley House, Dorchester.

1873 ALLEN, HENRY MARCUS, L.R.C.P. Ed., 38, Regency square, Brighton.

1859 AMSDEN, GEORGE JOHN, M.D., 85, St. Paul's road, Highbury, N.

1875 ANDERSON, JOHN FORD, M.D., C.M., 28, Buckland crescent, Belsize park, N.W.

1866 ANDREWS, HENRY CHARLES, M.D., 1, Oakley square, N.W.

1859 ANDREWS, JAMES, M.D., 149, Camden road, N.W.

1870 APPLETON, ROBERT CARLISLE, Toll Garel, Beverley.

1859 ARCHER, JOHN, F.R.C.S., 9, Carpenter road, Edgbaston, Birmingham.

Elected

1871 Argles, Frank, L.R.C.P. Ed., Hermon Lodge, Wanstead, Essex, N.E.

1861 Armstrong, John, M.D., Green street green, Dartford, Kent.

O.F. Aveling, James H., M.D., Physician to the Chelsea Hospital for Women; 1, Upper Wimpole Street, W. *Council*, 1865-66, 1872. *Hon. Sec.* 1873. *Hon. Lib.* 1874-6.

1872 Ayling, Arthur H. W., 103, Great Portland street, W.

1859 Ayling, William Henry, L.R.C.P. Ed., 103, Great Portland street, W.

1873 Bailey, James Johnson, M.D., L.R.C.P. Ed., Marple, Cheshire.

1875 Bailey, Thomas, Godstone, Surrey.

1864 Baker, G. Benson, 40, Hoghton street, Southport, Lancashire.

1867 Baker, John C., M.D., F.R.C.S. Ed.; 6, Gambier terrace, Hope street, Liverpool.

1859 Baker, John Wright, Surgeon to the Derbyshire General Infirmary; 102, Friar gate, Derby. *Hon. Local Sec.*

O.F. Bannister, John Henry, 436, Oxford street, W.

1869 Bantock, George Granville, M.D., Physician to the Samaritan Free Hospital; 44, Cornwall road, Westbourne park, W. *Council*, 1874-6.

1874 Barber, Edward, 105, Devonshire street, Sheffield.

1874 Barber, Oliver, 89, Cemetery road, Sheffield.

1874 Barclay, John, M.D., 10, Low street, Banff.

1863 Barker, Edward, F.R.C.S., Senior Surgeon to the Melbourne Hospital, Victoria.

O.F. Barnes, Robert, M.D., F.R.C.P., Obstetric Physician to, and Lecturer on Midwifery at, St. George's Hospital; 31, Grosvenor street, W. *Vice-Pres.* 1859-60. *Council*, 1861-62, 1867-76. *Treas.* 1863-64. *Pres.* 1865-66. *Trustee.*

1875 Barnes, R. S. Fancourt, M.B., C.M., 33, Weymouth street, Portland place, W.

1863 Barratt, Joseph G., M.D., 8, Cleveland gardens, Bayswater, W.

Elected

1871 Barrick, Eli James, M.D., Professor of Midwifery at Victoria University; 91, Bond street, Toronto, Ontario, Canada.

1861* Bartrum, John S., F.R.C.S., Surgeon to the Bath General Hospital; 41, Gay street, Bath. *Hon. Loc. Sec.*

1866 Bassett, John, M.D., Professor of Midwifery at the Queen's College, Birmingham; 144, Hockley Hill, Birmingham. *Council*, 1874-6.

1873 Bate, George P., L.R.C.P. Ed., 412, Bethnal Green road, E.

1859 Bateman, Henry, F.R.C.S., 13, Canonbury lane, Islington, N.

1867 Batten, Rayner W., M.D., Physician to the Gloucester General Infirmary; 1, Brunswick square, Gloucester. *Hon. Loc. Sec.*

1859 Battye, Richard F., M.R.C.P. Ed., 6, Gloucester street, Belgrave road, Pimlico, S.W.

1871 Beach, Fletcher, M.B., Asylum for Idiots, Lower Clapton, N.E.

1871 Beadles, Arthur, 11, Park road terrace, Forest hill, S.E.

O.F. Beck, T. Snow, M.D., F.R.S., 7, Portland place, W.

1868 Beigel, Hermann, M.D., 2, Lichtenstein Strasse, Vienna.

1866 Belcher, Henry, M.D., L.R.C.P. Ed.; 12, Pavilion parade, Brighton.

1871 Bell, Robert, M.D. Glasg., 50, Woodlands road, Glasgow.

1873* Bennet, James Henry, M.D., The Ferns, Weybridge, and Mentone.

O.F. Berry, Samuel, F.R.C.S., Consulting Surgeon-Accoucheur to the Queen's Hospital, and Professor of Midwifery and the Diseases of Women and Children in the Queen's College; Hatfield House, 281, Hagley road, Edgbaston, Birmingham. *Vice-Pres.* 1859. *Hon. Loc. Sec.*

1872 Bethune, Norman, M.D., Professor of Anatomy, Trinity College, Toronto.

Elected

1868 Black, James Watt, M.D., Obstetric Physician to the Charing Cross Hospital; 15, Clarges street, Piccadilly, W. *Council*, 1872-4.

1874 Blair, William, M.D., 9, Abbey green, Jedburgh.

1861* Blake, Thomas William, Hurstbourne Tarrant, Andover, Hants.

O.F. Blake, Valentine W., F.R.C.S., Surgeon to the Birmingham and Midland Counties Lying-in Hospital; 6, Old square, Birmingham.

1872 Bland, George, Surgeon to the Macclesfield Infirmary; Park Green, Macclesfield.

1866 Blease, Thomas, Clairville, Altrincham, Cheshire.

1860 Blood, Michael, 3, Boyne terrace, St. Helier's, Jersey.

O.F. Bloxam, William, M.D., 21, Mount street, Grosvenor square, W. *Council*, 1875.

1874 Boddaert, Gustav, M.D., Surgeon to the Civil Hospital, and to the Lunatic Asylum, Ghent, Belgium.

1868 Boggs, Alexander, M.D., late of H.M.'s Madras Army, 362, Rue St. Honoré, Paris.

1872 Bosworth, John Routledge, Sutton, Surrey.

1866 Boulton, Percy, M.D., Physician for Out-patients to the Samaritan Free Hospital; 6, Seymour street, Portman square, W.

1869 Boyd, Herbert, Assistant-Surgeon, Indian Army, Fort William, Calcutta [agents, Henry S. King and Co., 65, Cornhill].

1872 Bracey, Charles J., M.B., Surgeon to the Birmingham and Midland Hospital for Women; 71, Hagley road, Edgbaston, Birmingham.

1873 Braithwaite, James, M.D., Lecturer on Midwifery and Diseases of Women and Children at the Leeds School of Medicine; Assistant Surgeon to the Leeds Hospital for Women and Children; Little Woodhouse, Leeds.

Elected

1862 Braithwaite, William, M.D., late Lecturer on Midwifery, Leeds School of Medicine; Clarendon House, 20, Clarendon road, Leeds. *Council*, 1869-70.

1875 Branson, John, Rotherham, Yorkshire.

1875 Brewer, Alexander Hampton, 201, Queen's road, Dalston, E.

1862 Brickwell, John, Sawbridgeworth, Herts.

1872 Bridgewater, Thomas, M.B., Harrow-on-the-Hill, N.W.

1873 Briggs, George Chapman, Harrow-on-the-Hill, N.W.

1864 Bright, John Meaburn, M.D., The Glen, Forest hill, Sydenham, S.E. *Council*, 1873-74.

1869 Brisbane, James, M.D., 30, Lisson grove, N.W.

1860 Britton, William Samuel, 53, Wellington road, St. John's Wood, N.W.

1871 Brockman, Edward Forster, Assistant-Surgeon, General Hospital, Madras, and Professor of Pathology, Madras Medical College, Madras.

1866 Brodie, George B., M.D., 56, Curzon street, Mayfair, W. *Council*, 1873-75.

1868 Brown, Andrew, L.R.C.P. Ed., Elton villa, Bartholomew road, Kentish town, N.W.

1874 Brown, Charles Robert, M.D., Beckville, Beckenham, Kent.

1866 Brown, George Dransfield, Henley villa, Uxbridge road, Ealing, Middlesex.

1866 Brown, Thomas, M.D., 236, Kennington park road, S.E.

1865 Brunton, John, M.D., M.A., Surgeon to the Royal Maternity Charity; 21, Euston road, N.W. *Council*, 1871-3.

1863 Bryant, Thomas, F.R.C.S., Surgeon to Guy's Hospital 53, Upper Brook street, W. *Council*, 1866-67.

O.F. Bryant, Walter John, F.R.C.S., M.R.C.P. Ed., 23A, Sussex square, Hyde park gardens, W. *Council*, 1859.

Elected

1870 Buck, Joseph Randle, L.R.C.P. Ed., 26, Sidbury, Worcester.

1872 Buckingham, Charles E., M.D., Professor of Obstetrics in Harvard University; Consulting Physician to the Boston Lying-in Hospital; 53, Worcester street, Boston, Massachusetts, U.S.

1871 Bulmer, Thomas Sanderson, M.D. Toronto; Reefton Hospital, Province of Nelson, New Zealand. [*Per* Mr. Arthur Tabor, Solicitor, 27, Leadenhall street.]

1861 Bunny, Joseph, M.D., Hon. Surgeon to the Newbury Dispensary; Northbrook street, Newbury, Berks.

1867 Burnett, Charles, M.B., Biggleswade, Bedfordshire.

1866 Burrows, Sir John Cordy, F.R.C.S., Consulting Surgeon to the Brighton Hospital for Sick Children; 62, Old Steine, Brighton.

1862 Burton, John Mcülden, F.R.C.S., Lee park lodge, Lee, Kent, S.E *Council*, 1868-69.

1864 Butler, Frederick John, M.D., Surgeon to Winchester College and St. Cross Hospital, and to the Hants County Hospital, Winchester. *Hon. Loc. Sec.*

1870 Butler, John M., M.D., Physician to the Royal Kent Dispensary, and to the Woolwich Dispensary; 6, Queen's terrace, (28) Woolwich Common, S.E.

1868 Butt, William Frederick, L.R.C.P. Lond., 12, South street, Park lane, W. *Council*, 1876.

1861 Candlish, Henry, M.D., Physician to the Alnwick Infirmary, Bondgate street, Alnwick, Northumberland.

1861 Candy, John, M.D., Staff-Surgeon, Army Medical Department. [*Per* Messrs. Price and Boustead, Army Agents, 34, Craven street, Strand, W.C.]

1872 Carless, Edward Nicholls, M.B., C.M., Lansdowne grove, Devizes, Wilts.

1863 Carlyle, David, M.D., 2, The Crescent, Carlisle.

1861 Carter, Albert Pleydell, Wellington House, London road, Gloucester.

Elected

1872 Carter, Charles Henry, M.D., B.A., B.S., Assistant Physician to the Hospital for Women; 45, Great Cumberland place.

1869 Caskie, John Boyd, M.D., 89, Goswell road, E.C.

1869 Cass, William Cunningham, 20, St. George's road, S.W.

1870 Causton, William Henry, Hepworth lodge, 3, Westcroft place, Hammersmith.

1863 Cayzer, Thomas, Mayfield, Aigburth, Liverpool.

1873 Chadwick, James R., M.A., M.D., Lecturer on Diseases of Women, Harvard University; Physician to the Department of Diseases of Women, Boston City Hospital, and to the Dispensary for Diseases of Women; Marlboro' street, Boston, Massachusetts, U.S.

1875 Chaffers, Edward, F.R.C.S., 54, North street, Keighley, Yorkshire.

1873 Chalmers, John, M.D., 43, Caledonian road, N.

1864 Chambers, Thomas, F.R.C.P. Ed., F.R.C.S. Ed., Senior Physician to the Chelsea Hospital for Women, Consulting Physician Accoucheur to the Western Maternity; 64, Chester square, S.W. *Council*, 1874-6.

1859 Chance, Edward John, F.R.C.S., Surgeon to the Metropolitan Free Hospital and City Orthopædic Hospital; 59, Old Broad street, City, E.C.

1862 Chapman, Walter, F.R.C.S., Lower Tooting, Surrey.

1867* Charles, T. Edmondstoune, M.D., Professor of Midwifery at the Calcutta Medical College, 10, Harrington street, Calcutta, *Hon. Loc. Sec.*

1874 Charlesworth, James, 27, Birch terrace, Hanley, Staffordshire.

1865* Charlton, Egbert, M.D., Fareham, Hants.

1863 Chavasse, Pye Henry, F.R.C.S., Priory House, 12, The Square, Birmingham.

1868 Child, Edwin, New Malden, Kingston-on-Thames, Surrey.

1863* Chisholm, Edwin, Camden, near Sydney, New South Wales. [Per Henry K. Edge, Esq., 16, Philpot lane, E.C.].

Elected

1859 Claremont, Claude Clarke, Millbrook House, 1, Hampstead road, N.W.

1859 Clark, James Fenn, 18, York terrace, Leamington. *Hon. Loc. Sec.*

1874 Clark, James Henry, L.R.C.P. Ed., Long Hill, Goschen, P. O., St. Elizabeth, Jamaica.

1862 Clarke, John, Lynton, North Devon.

1872 Clarke, William Michell, late Surgeon to the British General Hospital; 2, York buildings, Clifton, Bristol.

O.F. Clay, Charles, M.D., late Lecturer on Midwifery and Clinical Medicine in St. Mary's Hospital, Manchester; Audenshaw Lodge, Audenshaw, near Manchester. *Council*, 1863-65.

O.F. Clay, John, Professor of Midwifery, Queen's College, Birmingham; Allan House, 138-9, Steelhouse lane, Birmingham. *Council*, 1868-69. *Vice-Pres.* 1872-4.

O.F. Cleveland, William Frederick, M.D., Stuart villa, 199, Maida vale, W. *Council*, 1863-64. *Vice-Pres.* 1875-76.

1861 Clogg, Stephen, East Looe, Cornwall.

1865* Coates, Charles, M.D., Physician to the Bath General and Royal United Hospitals; 10, Circus, Bath.

1860 Cockell, Edgar, Holly lodge, Forest road, Dalston, N.E.

1859 Cockell, Frederick Edgar, 144, Amherst road, Hackney, N.E.

1875 Coffin, Richard Jas. Maitland, M.R.C.P. Ed., Valetta, Malta.

1875 Cole, Richard Beverly, M.D. Jefferson Coll. Philad., San Francisco, California, U.S.

1873 Concanon, William Augustus, L.K.Q.C.P. Ireland; Ladway Villa, Pill, near Bristol, Somersetshire.

1866 Coombs, James, M.D., Bedford.

Elected

1864 Cooper, George Henry C., M.D., F.R.C.S. Ed., Surgeon to the Holloway and N. Islington Dispensary; Surgeon-Accoucheur to the Royal Maternity Charity; 35, Compton terrace, Islington, N.

1873 Cooper, Frank W., Leytonstone, Essex.

1874 Cooper, Herbert, L.R.C.P. Ed., Hampstead.

1861 Cooper, John, M.R.C.P. Ed., Clapham rise, S.W.

1872 Coote, Michael, M.D., 15, Ivanhoe terrace, Ashby-de-la-Zouch.

1865 Copeman, Edward, M.D., Physician to the Norfolk and Norwich Hospital; Upper King street, The Close, Norwich. *Council*, 1869-71. *Vice-Pres.*, 1873-5. *Hon. Loc. Sec.*

1875 Cordes, Aug., M.D., Professor of Obstetrics at the University of Geneva; 60, Rue du Rhone, Geneva.

1866 Cornwall, James, F.R.C.S., Fairford, Gloucestershire.

1860 Corry, Thomas Charles Steuart, M.D., Senior Surgeon to the Belfast General Dispensary; 146, Donegall Pass, Belfast. *Council*, 1867. *Hon. Loc. Sec.*

1859 Cory, Frederick Charles, M.D., Portland villa, Buckhurst hill, Essex. *Council*, 1867-69.

1875 Cory, Robert, M.B., Assistant Obstetric Physician to St. Thomas's Hospital; Palace road, Albert Embankment, S.E.

1863 Coward, John W., Christchurch, Canterbury, New Zealand [agents: Messrs. Arthur Hill and Sons, 101, Southwark street, S.E.]. *Hon. Loc. Sec.*

1869 Cox, Richard, L.R.C.P. Ed., Theale, near Reading.

1870 Craigie, John, M.D., 2, West Cliff cottages, Lyme Regis, Dorset.

1859 Croft, J. McGrigor A. T., M.D., M.R.C.P., 15, Abbey road, St. John's Wood, N.W.

1866 Croft, Robert Charles, L.R.C.P. Ed., 204, Camden road, N.W.

1874 Crombie, Charles Mann, M.B. & C.M., 9, Union terrace, Aberdeen.

Elected

1860 Cross, Richard, M.D., Carlton House, Belmont road, Scarborough, Yorkshire. *Hon. Loc. Sec.*

1869 Cross, Robert Shackleford, Petersfield, Hants.

1867 Croucher, Henry, West Hill, Dartford, Kent.

1875 Cullingworth, Charles James, Surgeon to St. Mary's Hospital, Manchester; 260, Oxford street, Manchester.

1859 Culpeper, William Moe, 1, Brunswick terrace, Palace gardens, Kensington, W.

1862 Cumberbatch, Laurence Trent, M.D., 25, Cadogan place, Belgrave square, S.W. *Council*, 1868-70.

1875 Cundell, George Richard, St. Mary's Hospital, Paddington.

1867 Cuolahan, Hugh, M.D., 9, Grange road, Bermondsey, S.E.

1859 Curgenven, J. Brendon, 11, Craven hill gardens, Bayswater, W. *Council*, 1870-72.

1868 Daly, Frederick Henry, M.D., 101, Queen's road, Dalston, N.E.

1870 Daniel, William Abbot, Kingston-on-Thames.

O.F. Davis, John Hall, M.D., F.R.C.P., Obstetric Physician to, and Lecturer on Midwifery and Diseases of Women and Children at, the Middlesex Hospital; Physician to the Royal Maternity Charity; Consulting Physician-Accoucheur to the St. Pancras Infirmary; 24, Harley street, Cavendish square, W. *Council*, 1859, 1864-65, 1869-76. *Vice-Pres.* 1861-63. *Pres.* 1867-68.

1863 Davis, Robert Alex., M.D., Resident Physician, County Asylum, Burntwood, near Lichfield, Stafford.

1873 Davison, Francis, L.R.C.P. Ed., Russel street, Armagh.

1859 Day, William Henry, M.D., Physician to the Samaritan Free Hospital for Women and Children; 10, Manchester square, W. *Council*, 1873-75.

1872 Denton, George Bagster, Surgeon to the Ladies' Charity and Lying-in Hospital; 2, Abercromby square, Liverpool.

1860 Dickenson, John, F.R.C.S., Hon. Surgeon to the Wrexham Infirmary; Wrexham, Denbighshire.

Elected

1859 Dixon, John, M.D., Melbourne Lodge, Anerley road, Anerley, S.E.

O.F. Drage, Charles, M.D., Hatfield, Herts. *Council*, 1861-4.

1875 Drew, Walter Henry, M.R.C.P. Ed., 2, Ospringe road, Kentish town, N.W.

O.F. Druitt, Robert, F.R.C.P., 8, Strathmore Gardens, Kensington Mall, W. [Madras.] *Council*, 1859-60. *Vice-Pres.* 1862-64.

1859 Druitt, William, F.R.C.S., Wimborne, Dorset.

O.F. Duncan, James, M.B., 8, Henrietta street, Covent garden, W.C. *Council*, 1873-74.

1859 Duncan, Peter Charles, M.D., 32, New Cross road, Hatcham Park, S.E.

O.F. Dunn, Robert, F.R.C.S., 31, Norfolk street, Strand, W.C. *Council*, 1860. *Vice-Pres.* 1861-62.

1871 Eastes, George, M.B., F.R.C.S., Surgeon-Accoucheur to the Western General Dispensary; 5, Albion place, Hyde park square, W.

1866 Easton, John, M.D., 19, Norfolk Crescent, Hyde park, W.

1867 Edis, Arthur W., M.D., Assistant-Physician-Accoucheur to the Middlesex Hospital; Physician to the British Lying-in Hospital; 22, Wimpole street, W. *Council*, 1873-74. *Hon. Sec.* 1875-76.

1861 Ellis, Robert, Obstetric Surgeon to the Chelsea, Brompton, and Belgrave Dispensary; 63, Sloane street, S.W. *Council*, 1868-70.

1862 Ellison, James, M.D., Surgeon to H.M.'s Household, Windsor; 14, High street, Windsor, Berks. *Council*, 1873-75.

1873 Engelmann, George Julius, A.M., M.D., 3003, Locust street, St. Louis, Missouri, U.S.

1873 Evans, Maurice G., M.D., Routh, Cardiff.

1871 Evans, Thomas Walter, 101, Heyworth street, Everton, Liverpool.

Elected

1875 Ewart, John Henry, Surgeon to St. Mary's Hospital for Women and Children; Limefield House, Cheetham hill, Manchester.

1875 Eyeley, Joseph Frederick, L.R.C.P. Lond., Parsonage street, Dursley, Gloucestershire.

1865 Fairbank, Thomas, M.D., Surgeon to H.M. the Queen and the Royal Household, Windsor Castle; Moulsey House, Sheet street, Windsor, Berks.

1859 Faircloth, Richard, F.R.C.S., Newmarket, Cambridgeshire.

1869 Farquhar, William, M.D., Surgeon-Major, Madras Army, Bangalore, Madras Presidency. [24, Pembroke gardens, Kensington.]

1861 Farr, Geo. F., L.R.C.P. Ed., Slade House, 175, Kennington road, S.E.

1868 Fegan, Richard, M.D., 1, Charlton park terrace, Old Charlton, Kent

1872 Fergusson, Alexander, F.R.C.S. Ed., Briarbank, Peebles, N.B.

O.F. Fergusson, Sir William, Bart., F.R.C.S., F.R.S., Sergeant-Surgeon to H.M. the Queen, Professor of Surgery in King's College and Surgeon to King's College Hospital, Consulting Surgeon to the Samaritan Free Hospital; 16, George street, Hanover square, W. *Vice-Pres.* 1862-63.

1872 Fernie, Henry Mortlock, Park green, Macclesfield, Cheshire.

1861 Fetherston, Gerald H., M.D.; Hon. Physician to the Melbourne Lying-in Hospital, Prahran, Melbourne, Victoria. *Hon. Loc. Sec.*

1873 Finegan, James Herbert, M.D., 72, Rodney street, Liverpool.

1870 Fisher, John Moore, M.D., 2, Balmoral terrace, Anlaby road, Hull.

1873 Fitch, Simon, M.A., M.D., Portland, Maine, U.S.

1874 Fitzrayne, William Allen, 134, Tooley street, S.E.

Elected

1868 Fletcher, Edward, Lygon street, Carlton, Melbourne, Victoria.

1865 Fowler, James, F.S.A., Hon. Surgeon to the Clayton Hospital and Wakefield General Dispensary; 13, South Parade, Wakefield. *Council*, 1872-4. *Hon. Loc. Sec.*

1866 Fox, Cornelius Benjamin, M.D., Medical Officer of Health, Maldon Union Sanitary District, Chelmsford, Essex.

1862 Frain, Joseph, M.D., Hon. Surgeon to the South Shields Dispensary; Frederick street, South Shields.

1861 Frankland, Thomas Thrush, Surgeon to the Ripon Dispensary, North House, Ripon, Yorkshire.

1875 Fraser, Angus, M.D., 232, Union street, Aberdeen.

1874 Fraser, Robert Maclean, L.R.C.P. Ed., 6, North terrace, Northgate, Darlington.

1867 Freeman, Henry W., 24, Circus, Bath.

1867 Fryer, Charles, L.K.Q.C.P. Ireland.

1867 Fuller, Charles C., 33, Albany street, Regent's park, N.W.

1874* Galabin, Alfred Lewis, M.A., M.D., Assistant-Physician to the Hospital for Sick Children; Assistant Obstetric Physician to Guy's Hospital; 14, St. Thomas's street, Southwark, S.E. *Council*, 1876.

1863 Galton, John H., M.D., Three Oak villa, Thicket road, Anerley, S.E. *Council*, 1874-6.

1872 Gardner, W., M.A., M.D., 525, St. Joseph street, Montreal.

1863 Garman, Henry Vincent, Kent House, Bow road, E.

1873 Garton, William, M.B., F.R.C.S., Hardshaw street, St. Helen's, Lancashire.

1859 Gaskoin, George, 7, Westbourne park, W. *Council*, 1870-72.

1875 Gawith, J. Jackson, St. Mary's Hospital.

1869 Geikie, Walter B., M.D., F.R.C.S. Ed., Professor of the Principles and Practice of Medicine and of Clinical Medicine in the University of Trinity College, Toronto, Ontario, Canada.

Elected

1859 Gervis, Henry, M.D., F.R.C.P., Obstetric Physician to, and Lecturer upon Obstetric Medicine at, St. Thomas's Hospital; Physician to the Royal Maternity Charity; 13, St. Thomas's street, Southwark, S.E. *Council*, 1864-66. *Hon. Sec.* 1867-70. *Vice-Pres.* 1871-3.

1866 Gervis, Frederick Heudebourck, 1, Fellowes road, Hampstead, N.W.

1875 Gibbings, Alfred Thomas, M.D., 93, Richmond road, Dalston, N.E.

1874 Gibson, James Edward, Bath road, West Cowes, Isle of Wight.

1866 Giddings, William Kitto, L.R.C.P. Ed., Shaftesbury House, Calverley, near Leeds, Yorkshire.

1875 Giles, Peter Broome, L.R.C.P. Ed., Staunton-on-Wye, Hereford.

1869 Gill, William, L.R.C.P. Lond., 11, Russell square, W.C.

1867 Gittins, John, L.R.C.P. Ed., St. Olave's Union, Parish street, Southwark, 134, Tooley street, S.E.

1871 Goddard, Eugene, L.R.C.P. Lond., 27, Pentonville road, N.

1869 Godden, Joseph, L.K.Q.C.P. Ireland; 47, Gildringe road, Eastbourne.

1871 Godson, Clement, M.D., C.M., Assistant Physician-Accoucheur to St. Bartholomew's Hospital; Physician to the Samaritan Free Hospital; 8, Upper Brook street, Grosvenor square, W. *Council*, 1876.

1868 Godwin, Ashton, M.D., 28, Brompton crescent, Brompton, S.W.

1873 Goldsmith, John, M.D., Highworth House, Liverpool gardens, Worthing, Sussex.

1873 Goodchild, Nathaniel, L.R.C.P. Ed., 9, Highgate road, N.W.

1872 Gornall, John Hankinson, Surgeon to the Warrington Dispensary; Friars Green House, Warrington.

1869 Goss, Tregenna Biddulph, 31, The Paragon, Bath.

1875 Gray, James, M.D., 15, Newton terrace, Glasgow.

Elected

1873 Gray, Robert, L.K.Q.C.P.I., Scotch street, Armagh, Ireland.

1874 Greene, William Thomas, M.A., L.K.Q.C.P.I., 218, Old Kent road, S.E.

1863 Griffith, G. de Gorrequer, Physician to the Hospital for Women and Children, Pimlico; Physician-Accoucheur to St. Saviour's Maternity; 9, Lupus street, St. George's square, S.W.

1869 Griffith, John T., M.D., Talfourd House, Camberwell.

1859 Griffith, Thomas Taylor, F.R.C.S., Consulting Surgeon to the Wrexham Infirmary; Wrexham, Denbighshire. *Council*, 1870-72.

1870 Grigg, William Chapman, M.D., Physician to the In-patients, Queen Charlotte's Lying-in Hospital; Assistant Obstetric Physician to the Westminster Hospital; Assistant-Physician to the Victoria Hospital for Children; 6, Curzon street, Mayfair. *Council*, 1875-76.

O.F. Grimsdale, Thos. F., L.R.C.P. Ed., Consulting Surgeon to the Lying-in Hospital, and late Lecturer on Diseases of Children, &c., at the Royal Infirmary School of Medicine; 29, Rodney street, Liverpool. *Council*, 1861-62. *Vice-Pres.* 1875-76.

1865 Gwyn, George F., Westcroft House, Hammersmith, W.

1867 Hadaway, James, L.R.C.P. Ed., 47B, Welbeck street, Cavendish square, W.

O.F. Hall, Alfred, M.D., Senior Physician to the Brighton Dispensary; 30, Old Steyne, Brighton. *Council*, 1864-65. *Vice-Pres.* 1866-68. *Hon. Loc. Sec.*

1859 Hall, Frederick, 1, Jermyn street, St. James's, S.W.

1867 Hall, John Henry Wynne, L.R.C.P. Ed., 118, Wandsworth road, S.W.

1862 Hall, William, Hillary place, Leeds. *Hon. Loc. Sec.*

1871 Hallowes, Frederick B., Redhill, Reigate, Surrey.

1874 Hannan, Francis John, M.B., Knaphill, Woking, Surrey.

1860 Hardey, Key, Surgeon to the West City Dispensary; 4, Wardrobe place, Doctors' Commons, E.C.

Elected

1869 HARDINGE, HENRY, M.D., Physician to the Great Northern Hospital; 18, Grafton street, Bond street, W.

1872 HARDING, WILLIAM, F.R.C.S., 4, Percy street, Bedford square, W.C.

O.F. HARPER, PHILIP H., F.R.C.S., 30, Cambridge street, Hyde park, W.

O.F. HARRINSON, ISAAC, F.R.C.S., Castle street, Reading, Berks. *Council*, 1862-65. *Hon. Loc. Sec.*

1862 HARRIS, CHARLES, M.D., Northiam, Ashford, Kent.

1871 HARRIS, CHARLES JAMES, 11, Kilburn Priory, N.W.

1872 HARRIS, HENRY, M.D., F.R.C.S., Trengweath place, Redruth, Cornwall.

1867 HARRIS, WILLIAM H., M.D., Professor of Midwifery in the Madras Medical College, and Superintendent of the Lying-in Hospital, Madras [agent: Mr. H. K. Lewis, Gower street].

1861 HARRIS, WILLIAM JOHN, 13, Marine Parade, Worthing. *Hon. Loc. Sec.*

1865 HARVEY, ROBERT, M.D., Civil Surgeon of Bhurtpore, near Agra, India [*viâ* Bombay]. [Per Messrs. Cochran and Anderson, 152, Union street, Aberdeen.]

1873 HATHERLY, HENRY REGINALD, L.R.C.P. Ed., Wellington street, Park side, Nottingham.

1868 HAY, THOMAS BELL, L.R.C.P. Ed.; 43, Caledonian road, N.

1865 HAYES, HAWKESLEY ROCHE, Basingstoke, Hants.

1873 HAYES, THOMAS CRAWFORD, M.B., Assistant Obstetric Physician to King's College Hospital; 17, Clarges street, Piccadilly, W. *Council*, 1876.

1867 HEMBROUGH, JOHN WILLIAM, Ivy cottage, Waltham, Grimsby.

1870 HENDERSON, ALEXANDER, 2, Meadow Bank place, Rose vale, Partick, Glasgow.

1860 HESS, AUGUSTUS, M.D., Physician to the Jews' Hospital, Norwood; 14, Artillery place, Finsbury square, E.C.

Elected

O.F. Hewitt, Graily, M.D., F.R.C.P., Professor of Midwifery in University College, London, and Obstetric Physician to University College Hospital; 36, Berkeley square, W. *Hon. Sec.* 1859-64. *Treas.* 1865-66. *Vice-Pres.* 1867-68. *Pres.* 1869-70. *Council*, 1871-76.

1862 Hewitt, Tom Smith, M.D., Ivy Cottage, Winkfield, Windsor, Berks.

1867 Hickinbotham, James, L.R.C.P. Ed., 10, Park road, Nechells, Birmingham.

1860 Hicks, John Braxton, M.D., F.R.C.P., F.R.S., Physician-Accoucheur to, and Lecturer on Midwifery and the Diseases of Women and Children at, Guy's Hospital; Physician to the Royal Maternity Charity; 24, George street, Hanover square. *Council*, 1861-2, 1869, 1873-76. *Hon. Sec.* 1863-65. *Vice-Pres.* 1866-68. *Treas.* 1870. *Pres.* 1871-2.

1860 Higgs, Thomas Frederic, L.R.C.P. Ed., 194, High street, Dudley, Worcestershire.

1872 Hilliard, Robert Harvey, M.D., 5, Belgrave terrace, Upper Holloway, N.

1868 Hime, Thomas Whiteside, M.B., 217, Glossop road, Sheffield.

1865 Hodder, Edward M., M.D., F.R.C.S, Professor of Obstetrics, &c., Trinity College, Toronto; Consulting Surgeon, Toronto General Hospital; and Senior Physician-Accoucheur to the Burnside Lying-in Hospital; Toronto, Canada West. *Hon. Loc. Sec.*

O.F. Hodges, Richard, M.D., F.R.C.S., 16, Orchard street, Portman square, W.

1864 Hoffmeister, William Carter, M.D., Surgeon to the Queen in the Isle of Wight; Clifton House, Cowes, Isle of Wight.

1871 Hogg, Francis Roberts, M.D., India.

1874 Hoggan, George, M.B., 13, Granville Place, Portman square, W.

1875 Hollings, Edwin, L.R.C.P. Ed., 36, Euston square, N.W.

Elected

1859 Holman, Constantine, M.D., Barons, Reigate, Surrey. *Council*, 1867-69. *Vice-Pres.* 1870-71.

1860 Holman, Henry Martin, M.D., Hurstpierpoint, Sussex.

1864 Hood, Wharton Peter, M.D., 65, Upper Berkeley street, Portman square, W.

1872 Hope, William, M.D., Physician to Queen Charlotte's Lying-in Hospital; 5, Bolton row, Mayfair, W.

1874 Hopkins, Alfred Boyd, 180, Shoreditch, E.

1861 Horton, George Edward, Castle street, Dudley, Worcestershire.

1864 Houghton, Henry George, L.K.Q.C.P. Ireland; 6, Mount street, Grosvenor square, W.

1859 Hutchinson, Jonathan, F.R.C.S., Surgeon to the London Hospital; 15, Cavendish square, W. *Council*, 1869-71.

1870 Huthwaite, Charles, L.R.C.P. Ed., Stratford square, Nottingham.

1861 Hutton, Charles, M.D., Physician to the General Lying-in Hospital; 26, Lowndes street, Belgrave square, S.W.

1859 Ilott, James William, Beechfield, Bromley, Kent.

1864 Jackson, Edward, M.B., Surgeon to the Sheffield Hospital for Women; Fern Bank, Glossop road, Sheffield. *Hon. Loc. Sec.*

1864 Jackson, Robert, M.D., 53, Notting hill square, W.

1873 Jakins, William Vosper, L.R.C.P. Ed., Sturt street, Ballarat, Victoria. [Per Isaac N. Jakins, Esq., 32, Osnaburgh street, Regent's park.]

1872 Jalland, Robert, Horncastle, Lincolnshire.

1872 James, Edwin Matthews, Surgeon to the Melbourne Hospital, Victoria; 169, Collins street east, Melbourne.

1862 Jay, Frederick Fitzherbert, Woodbury, Exeter, Devon.

1863 Jenkins, Robert W., Silver street, Axminster, Devonshire.

1859 Jennings, Joseph C. S., Abbey House, Malmesbury, Wilts.

Elected

1860 JEPSON, HENRY, F.R.C.S., Surgeon to the Kingston Dispensary; Hampton, Middlesex, S.W.

1861 JONES, EDWARD, M.D., 48, Sydenham Park, Kent.

1868 JONES, EVAN, Ty-Mawr, Aberdare, Glamorganshire.

1859 JONES, GEORGE, 12, New Hall street, Birmingham.

1868 JONES, JOHN, 60, King street, Regent street, W.

1874 JONES, JOHN THOMAS, L.K.Q.C.P. I., Brookside, Llanfyllin, Montgomeryshire. [Oxford House, St. John's road, Brixton, S.W.]

1873 JONES, PHILIP W., Baker street, Enfield, Essex.

1873 JONES, T. EYTON, Surgeon to the Denbigh Infirmary; The Priory, Wrexham.

1870 JONES, WILLIAM, Oak villa, Glynneath, Neath, Glamorganshire.

1868 JORDAN, WILLIAM ROSS, Surgeon to the Birmingham Hospital for Women; Manor House, Moseley, near Birmingham.

1875 JUKES, AUGUSTUS, M.B. Trin. Coll. Toronto; St. Catherine's, Ontario.

O.F. KEELE, GEORGE THOMAS, 81, St. Paul's road, Highbury, N.

1874 KEMPSTER, WILLIAM HENRY, L.R.C.P. Ed., Oak House, Bridge Road, Battersea.

1872 KENNEDY, JOHN EDWARD, M.B., Lecturer on Medical Jurisprudence, Trinity College, Toronto; 242, Richmond street, West Toronto, Canada.

1865* KERNOT, GEORGE CHARLES, M.D., 5, Elphinstone road, Hastings, Sussex.

1872 KERR, NORMAN S., M.D., F.L.S., 42, Grove road, Regent's park, N.W.

O.F. KIALLMARK, HENRY WALTER, 66, Prince's square, Westbourne grove, W.

1869 KINGSFORD, C. DUDLEY, M.D., Upper Clapton, N.E.

1860 KINGSFORD, EDWARD, F.R.C.S., Surgeon to the Sunbury Dispensary; Sunbury, Middlesex.

1862 KIRKPATRICK, JOHN RUTHERFORD, M.B. Dubl., Examiner in Midwifery, Royal College of Surgeons, Ireland; 4, Upper Merrion street, Dublin. *Council*, 1872-4.

Elected

1872* Kisch, Albert, 2, Circus place, Finsbury, E.C.

1867 Knaggs, Henry Guard, M.D., 72, Kentish Town road, N.W.

1874 Koch, Edwin L., M.D., Lecturer on Midwifery in the Ceylon Medical School, Columbo, Ceylon.

1872 Konrád, Mark, M.D., The Vienna Hospital, Vienna, Austria; and Gross-Wardein, Hungary.

1867 Langford, Charles P., 187, Goswell road, E.C.

O.F. Langmore, John Charles, M.B., F.R.C.S., 20, Oxford terrace, Hyde park, W. *Council*, 1861-64. *Vice-Pres.* 1869-71.

1866 Langston, Thomas, L.R.C.P. Ed., 29, Broadway, Westminster, S.W.

1862 Lanphier, Richard, M.B. Dubl., Alford, Lincolnshire.

1872 Lattey, James, 8, St. Mary Abbott's terrace, Kensington, W.

1875 Lawrence, Alfred Edward Aust, M.D., Physician-Accoucheur to the Bristol General Hospital; 15, Richmond hill, Clifton, Bristol.

1873 Lawton, Herbert A., Longfleet, Poole, Dorsetshire.

1867 Leaf, Walter, Surgeon to the St. Marylebone General Dispensary; 14, Furnival's Inn, Holborn, E.C.

1860 Leishman, William, M.D., Physician to the University Lying-in Hospital, Regius Professor of Midwifery in the University of Glasgow; 11, Woodside crescent, Glasgow. *Council*, 1866-68. *Vice-Pres.* 1869-70.

1872 Leonard, Crosby, Surgeon to the Bristol Royal Infirmary; Rockleigh House, White Ladies road, Clifton, Bristol.

1868 Leslie, William Burnup, M.D., Stonehaven, Kincardineshire.

1874 Lewis, Charles Francis, L.R.C.P. Ed., Henfield, Sussex.

1874 Lewis, Henry Harman Dendy, 120, Drummond street, Euston square, N.W.

1875 Liebman, Carlo, M.D. Vienna, Principal Surgeon, Trieste Civil Hospital, Trieste, Austria.

Elected

1873 Lindsay, W. B., M.D., Strathroy, Ontario, Canada.

1874 Lithgow, Robert Alexander Douglas, L.R.C.P. Ed., 13, North Brink, Wisbeach, Cambridgeshire.

1868 Llewellyn, Evan, L.R.C.P. Ed., 9, Mount place, London Hospital, E.

1872* Lock, John Griffith, M.A., Lansdowne House, Tenby.

1859 Lombe, Thomas Robert, M.D., Bemerton, Torquay.

1870 Long, Mark, M.D., 185, Amhurst road, Hackney, N.E.

1874 Longrigg, Dean, Dulwich road, Penge, S.E.

1874 Louttit, James, M.D., Trafalgar road, Greenwich, S.E.

1866 Lovegrove, Charles, M.D., The Oaks, Hythe, Kent.

1872 Lovegrove, James F., Ightham, Sevenoaks, Kent.

1862 Lowe, George, F.R.C.S., Burton-on-Trent, Staffordshire.

1869 Lowndes, Frederick Walter, 62, Mount Pleasant, Liverpool.

1866 Lucey, William Cubitt, M.D., Norfolk villas, Junction road, Upper Holloway, N.

1873 Lush, William John Henry, L.R.C.P.Ed., Fyfield House, Andover.

1869 Lydall, Wykeham H., L.R.C.P. Ed., 19, Mecklenburgh square, W.C.

1868 Lynch, J. Roche, 41, Chepstow villas, Westbourne grove.

1871 McBeath, William, M.D., Witham, Alford, Lincolnshire.

1871 McCallum, Duncan Campbell, M.D., Professor of Midwifery and Diseases of Women and Children, McGill University; Physician to the University Lying-in Hospital; and Physician to the Montreal General Hospital; Montreal, Canada.

1874 McCallum, John Henry, M.B. Toronto, Resident Physician at the Toronto General Hospital, Canada.

1871 M'Conkey, Thomas Clarkson, M.D., McGill Univ., M.R.C.S.E; Barrie, Ontario, Canada.

1873 Macdonald, John Alexander, M.D., Woburn, Beds.

Elected

O.F. Mackinder, Draper, M.D., Consulting-Surgeon to the Gainsborough Dispensary; Gainsborough, Lincolnshire. *Council*, 1871-3.

1872 McMonagle, Joseph, M.D., New Brunswick, Dominion of Canada.

1872 MacMordie, William Kirkpatrick, M.D., 1, College square east, Belfast.

1861 McVeagh, Dennis, L.K.Q.C.P. Ireland, 33, Bishop street, Coventry, Warwickshire.

1866 Maddever, John Coombe, M.D., 19, Battery place, Rothesay.

1859 Madge, Henry M., M.D., 4, Upper Wimpole street, W. *Council*, 1863-65. *Vice-Pres.* 1872-4.

1871 Malins, Edward, M.D., Hon. Medical Officer to the Birmingham Lying-in Charity; 8, Old Square, Birmingham.

1871 Manby, Alan Reeve, East Rudham, Brandon, Norfolk.

1868 March, Henry Colley, M.D., 2, West street, Rochdale.

1860 Marley, Henry Frederick, Padstow, Cornwall.

1859 Marley, Richard, 100, Trafalgar road, Old Kent road, S.E.

1862 Marriott, Robert Buchanan, Swaffham, Norfolk.

O.F. Marshall, John Brake, Nightingale road downs, Clapton, N.E.

1871 Martin, Edward, Senior Surgeon to the Weston-super-Mare Hospital; Victoria House, Weston-super-Mare.

1873 Martin, Henry Charrington, M.B., C.M., 11, Somers place, Hyde park, W.

1864 Martin, Lawrence J., M.D., Physician to the Melbourne Lying-in Hospital; 126, Collins street east, Melbourne, Victoria.

1875 Mason, John Wallis, 1, Osnaburgh terrace, Regent's park, W.

1866 Mattei, Antoine, M.D., Professor of Midwifery, Rue Thérèse 4, Paris.

Elected

1861 MATTHEWS, JOHN, M.D., 4, Mylne street, Myddelton square, E.C.

1867 MAY, HENRY, L.R.C.P. Lond., Fairfield House, Lichfield road, Aston, Birmingham.

O.F. MEADOWS, ALFRED, M.D., Physician-Accoucheur to, and Lecturer on Midwifery at, St. Mary's Hospital; 27, George street, Hanover square, W. *Council*, 1862-64. *Hon. Sec.* 1865-66. *Hon. Lib.* 1865. *Treas.* 1867-69. *Vice-Pres.* 1874-6.

1875 MILES, ABIJAH J., M.D., Professor of Diseases of Women and Children in the Cincinnati College of Medicine, Cincinnati, Ohio, U.S.

1871 MILLER, HUGH, M.D., Physician-Accoucheur to the Glasgow Maternity Hospital; 298, Bath crescent, Bath street, Glasgow.

1871 MILLER, JOHN FAURE, M.D., 28, Rue de Matignon, Faubourg St. Honoré, Paris. *Hon. Loc. Sec.*

1869 MILWARD, JAMES, 27, Charles Street, Cardiff.

1869 MINNS, PEMBROKE R. J. B., M.D., Thetford, Norfolk.

1867 MITCHELL, ROBERT NATHAL, M.D., 1, Amersham park villas, New Cross, Kent.

1873 MONCKTON, MARSHALL, L.F.P.S. Glasg., Hurstpierpoint, Sussex.

1871 MOODELLY, CHITTATHORE BANLOO, Graduate in Medicine and Surgery of the Madras Medical College; Officiating Gillah Surgeon, of Cochin; Trichinopoly.

1873 MOON, ROBERT HENRY, F.R.C.S., Fern Lodge, Lower Norwood.

1869 MOORE, JOSEPH, M.D., Counser villa, Balham, S.W.

1859 MOORHEAD, JOHN, M.D., Surgeon to the Weymouth Infirmary and Dispensary; Weymouth, Dorset.

1863 MORGAN, EDWARD, 15, Park street, Llanelly, Caermarthenshire.

1869 MORGAN, W. H., Surgeon 23rd Regiment, Tinilon, Travancore.

Elected

1871 Morrison, John R., L.R.C.P. Ed., 57, Cannon street road, E.

1875 Morton, Richard John, Aylsham, Norfolk.

O.F. Murray, Gustavus Charles P., M.D., Obstetric Physician to the Great Northern Hospital; 66, Great Cumberland place, Hyde park, W. *Council*, 1864-65. *Hon. Sec.* 1866-69. *Vice-Pres.* 1870-72. *Treas.* 1873-76.

O.F. Musgrave, Johnson Thomas, L.R.C.P. Ed., Irlam villa, 39, Finchley road, N.W. *Council*, 1859-60.

1875 Musgrave, Reginald Vernon, St. Mary's Hospital.

1859 Napper, Albert, Broad Oak, Cranleigh, Guildford, Surrey. *Council*, 1866-68.

1863 Nason, John James, M.B. Lond., 11, Bridge street, Stratford-on-Avon.

1859 Nason, Richard Bird, Nuneaton, Warwickshire.

1859 Neal, James, M.D., late Hon. Surgeon to the Lying-in Hospital, Birmingham; Barcelona House, Sandown, Isle of Wight.

1866 Neild, James Edward, M.D., Lecturer on Forensic Medicine, Melbourne University; 166, Collins street east, Melbourne.

1859 Newman, William, M.D., Surgeon to the Stamford and Rutland Infirmary; Barn Hill House, Stamford, Northamptonshire. *Council*, 1873-75. *Vice-Pres.* 1876.

1872 Niblett, Francis D., L.R.C.P. Ed., The Grove, Hackney, N.

1868 Nicholls, James, M.D., Duke street, Chelmsford, Essex.

1861 Nichols, George W., Augusta House, Rotherhithe, S.E.

1873 Nicholson, Arthur, M.B. Lond., King's College Hospital.

1868 Oates, Parkinson, M.D., 164, Cambridge street, Eccleston square, S.W.

O.F. Oldham, Henry, M.D., F.R.C.P., Consulting Obstetric Physician to Guy's Hospital; 4, Cavendish place, Cavendish square, W. *Vice-Pres.* 1859. *Council*, 1860, 1865-66, 1868-76. *Treas.* 1861-62. *Pres.* 1863-64. *Trustee.*

Elected

1859 Oldham, James, F.R.C.S., Consulting-Surgeon to the Brighton Lying-in Institution; 53, Norfolk square, Brighton. *Council*, 1866-68.

1869 Ord, George Rice, Streatham hill, Surrey.

1875 Orpen, Arthur Herbert, A.B., Birch Lodge, Ross, Herefordshire.

1863 Oswald, James Waddell Jeffries, L.R.C.P. Ed., 245, Kennington road, S.E.

1875 Owen, William, 201, Bethnal green road, E.

1869 Oxley, Martin G. B., L.K.Q.C.P.I., 80, Rodney street, Liverpool.

1859 Palfrey, James, M.D., Obstetric Physician to, and Lecturer on Midwifery and the Diseases of Women and Children at, the London Hospital; Physician to the General Lying-in Hospital; 29, Brook street, Grosvenor square, W.

1873 Parker, Robert William, 8, Old Cavendish street, W.

1867 Parks, John, The Wylde, Bury, Lancashire.

1873 Parks, Luther, A.M., M.D., Boston, United States. [Agents: Messrs. McCalmont Brothers & Co., Bankers, 15, Philpot lane, E.C.]

1872 Parr, George, 18, Upper Phillimore place, Kensington, W.

1874 Paterson, Alexander, M.D., Bahia, Brazil [*per* Mr. Alfred Good, New Poultry Chambers, 7, Poultry, London.]

1865* Paterson, James, M.D., Hayburn Bank, Partick, Glasgow.

1875 Paul Ernest Watson, L.K.Q.C.P.I., The Green, Calne, Wiltshire.

1874 Payne, William S. Hele, 87, Queen's Road, Peckham, S.E.

1864 Pearson, David Ritchie, M.D., 23, Upper Phillimore place, Kensington, W.

1871 Pedler, George Henry, 6, Trevor terrace, Rutland gate, S.W.

Elected

O.F. Peirce, Richard King, Surgeon to the Notting hill and Shepherd's Bush Dispensary, 16, Norland place, Notting hill, W.

1873 Perez, Diego, M.D., Montevideo, South America [*per* A. K. Mackinnon, Esq., care of Messrs. John Clinch & Sons, 9, Austin Friars, E.C.]

1871 Perrigo, James, M.D., 591, St. Catherine street, Montreal, Canada. *Hon. Loc. Sec.*

1871 Philps, Philip George, 4, Queen's road, Peckham, S.E.

1874 Pigg, Thomas, M.D., Physician to the Manchester Southern Hospital for Women and Children; 75, Piccadilly, Manchester.

1866 Pilcher, William John, 43, High street, Boston, Lincolnshire.

1864 Playfair, W. S., M.D., F.R.C.P., Professor of Obstetric Medicine in King's College, and Obstetric Physician to King's College Hospital; 31, George street, Hanover Square, W. *Council*, 1867. *Hon. Librarian*, 1868-9. *Hon. Sec.* 1870-72. *Vice-Pres.*, 1873-6.

O.F.* Pollard, William, Surgeon to the Torbay Hospital; Southlands, Torquay, Devon.

1864 Potter, John Baptiste, M.D., Obstetric Physician to the Westminster Hospital; 20, George street, Hanover square, W. *Council*, 1872-6.

1859 Pound, George, Odiham, Hants.

1875 Powdrell, John, 75, Euston road, N.W.

1863 Powell, Josiah T., M.D., 347, City road, E.C.

1864 Price, William Nicholson, Lecturer on Midwifery and the Diseases of Women and Children at the Leeds School of Medicine; 7, East parade, Leeds. *Council*, 1876.

1863 Price, William Preston, M.D., Surgeon to the Metropolitan Infirmary for Scrofulous Children, Margate; 1, Ethelbert Crescent, Margate. *Hon. Loc. Sec.*

Elected

O.F. PRIESTLEY, WILLIAM O., M.D., F.R.C.P., Consulting Obstetric Physician to King's College Hospital; and Consulting Physician-Accoucheur to the St. Marylebone Infirmary; 17, Hertford street, Mayfair, W. *Council*, 1859-61, 1865-66. *Vice-Pres.* 1867-69. *Pres.* 1875-76.

O.F. RADFORD, THOMAS, M.D., Consulting Physician to St. Mary's Hospital, Manchester; Moor field, Higher Broughton, Manchester. *Vice-Pres.* 1859.

1859 RAMSAY, JOHN ALLEN, L.R.C.P. Ed., Great Shelford, Cambridge.

O.F. RANDALL, JOHN, M.D., Lecturer on Medical Jurisprudence, St. Mary's Hospital Medical School; Medical Officer, St. Marylebone Infirmary; 35, Nottingham place, W.

1872 RANKIN, WILLIAM BAILY, Surgeon to Prince Alfred Hospital; High street, St. Kilda, Melbourne.

1861 RASCH, ADOLPHUS A. F., M.D., Physician for Diseases of Women to the German Hospital; Physician to the Training Hospital, Tottenham; 7, South street, Finsbury square, E.C. *Council*, 1871-3.

1870 RAY, EDWARD REYNOLDS, Dulwich.

1860* RAYNER, JOHN, M.D., Swaledale House, Quadrant road north, Highbury New Park, N.

1859 RAYNES, HENRY, Gringley-on-the-hill, Bawtry, Yorkshire.

1871 READ, CHARLES, M.B., 1, St. George's square, Regent's park road, N.W.

1874 REES, WILLIAM, Priory House, Queen's Crescent, Haverstock hill, N.W.

1875 REID, MATTHEW, L.R.C.P. Ed., 97, Blue Anchor road, Bermondsey, S.E.

O.F. REMINGTON, THOMAS, M.D., Visiting Medical Officer to the S. Lambeth and Brixton Dispensary; Alexandra Lodge, 89, Angell road, Brixton, S.W.

1875* REY, EUGENIO, M.D. of Turin, 92, Capole case, Rome.

1862 RICHARDS, DAVID, 8, St. George's place, Brighton, Sussex.

1859 RICHARDS, SAMUEL, M.D., 36, Bedford square, W.C. *Council*, 1864-66.

Elected

1862 Richards, S. Smith C., 36, Bedford square, W.C.

O.F. Richardson, Richard, L.R.C.P. Ed., Bryngwy, Rhayader, Radnorshire.

1872 Richardson, William L., M.D., A.M., Instructor in Clinical Obstetrics in Harvard University; Visiting Physician to the Boston Lying-in Hospital; 76, Boylston street, Boston, Massachusetts, U.S.

1871 Rickard, Frederick Martyn, Assistant-Surgeon 25th Madras Native Infantry, Cavanore.

1872 Rigden, George, Surgeon to the Canterbury Dispensary; Burgate street, Canterbury.

1871 Rigden, Walter, 8, Montpellier square, S.W.

O.F.* Roberts, David Lloyd, M.D., Physician to St. Mary's Hospital, Manchester; 23, St. John's street, Deansgate, Manchester. *Council*, 1868-70. *Vice-Pres.* 1871-2. *Hon. Loc. Sec.*

1867 Roberts, David W., M.D., 56, Manchester street, Manchester square, W.

1860 Roberts, Robert Price, Shamrock House, Rhyl, Flintshire.

1874 Robertson, William Borwick, M.D., West Dulwich, S.E.

O.F. Robinson, Thomas, M.D., 35, Lamb's Conduit street, W.C.

1875 Rogers, Amos, M.D. Ottawa, L.R.C.P. Ed.; P. O. Box 110, Ottawa, Ontario, Canada.

O.F. Rogers, William Richard, M.D., Physician to the Samaritan Free Hospital; 56, Berners street, Oxford street, W. *Council*, 1870-72.

1874 Roots, William Henry, Kingston-on-Thames.

O.F. Roots, William Sudlow, F.R.C.S., F.L.S., Surgeon to the Royal Establishment at Hampton Court, Kingston-on-Thames.

1860 Roper, Alfred George, 57, North End, Croydon, Surrey. *Hon. Loc. Sec.*

1874 Roper, Arthur, 17, Granville park, Blackheath.

1865 Roper, George, M.D., Physician to the Royal Maternity Charity; 6, West street, Finsbury circus, E.C. *Council*, 1875-76.

Elected

1859 Rose, Henry Cooper, M.D., High street, Hampstead, N.W. *Council*, 1875-6.

O.F. Routh, Charles Henry Felix, M.D., Physician to the Samaritan Free Hospital for Women and Children; 52, Montagu square, W. *Council*, 1859-61. *Vice-Pres.* 1874-6.

1874 Rowan, Thomas, L.R.C.P. Ed., Resident Surgeon, Lying-in Hospital, Melbourne, Victoria.

1870 Russell, Logan D. H., M.D., New Atlanta, Georgia, U.S.

1866 Saboia, V., M.D., Rio de Janeiro.

1864 Salter, John H., D'Arcy House, Tolleshunt D'Arcy, Kelvedon, Essex.

1875 Saltzmann, Frederick William, 18, Montpellier road, Brighton.

1868* Sams, John Sutton, St. Peter's Lodge, Eltham road, Lee, Kent.

1872 Sangster, Charles, 15, Lambeth terrace, S.E.

1875 Satchell, Walter Alfred, M.R.C.P. Ed., Kew, Surrey.

1870 Saul, William, M.D., 4, Charlotte street, Fitzroy square, W.

1863 Savage, Henry, M.D., Consulting Physician to the Samaritan Hospital for Women, Lower Seymour street, Portman square; 14, Bentinck street, W. *Council*, 1871-2.

1872 Savage, Thomas, M.D., Surgeon to the Birmingham and Midland Hospital for Women; 12, Old square, Birmingham.

O.F. Scott, John, F.R.C.S., 49, Harley street, Cavendish square, W. *Council*, 1868-70. *Vice-Pres.* 1871-3.

1870 Scott, John, M.D., New street, Sandwich.

1873 Sell, Edward H. M., M.A., M.D., 115, West Thirty-second street, New York City, U.S.

1863 Sequeira, Henry Little, 1, Jewry street, Aldgate, E.C.

1866 Sequeira, James Scott, 34, Leman street, Goodman's fields, E.

Elected

1875 Seton, David Elphinstone, M.D., 12, Thurloe place, South Kensington.

1860 Sewell, Charles Brodie, M.D., 76, Guilford street, Russell square, W.C., and 13, Fenchurch street, E.C.

1873 Seymour, Francis, Odiham, Hants.

1862 Sharman, Malim, Surgeon to the Birmingham Free Hospital for Sick Children; 18, New Hall street, Birmingham.

O.F. Sharpin, Henry Wilson, F.R.C.S., Surgeon to the Bedford General Infirmary, Bedford. *Council*, 1871-3.

1869 Shaw, Henry Sissmore, 88, Upgate, Louth, Lincolnshire.

O.F. Shearman, Edward James, M.D., F.R.C.S., F.R.S. Ed., Consulting Physician to the Rotherham Hospital; Moorgate, Rotherham, Yorkshire.

1859 Sheehy, William Henry, L.R.C.P. Ed., 4, Claremont square, Pentonville, N.

1875 Sheldon, Edwin Mason, Surgeon to Stanley Hospital; 223, Boundary street, Liverpool.

1867 Shepherd, Frederick, L.R.C.P. Ed., 33, King Henry's road, Primrose hill, N.W.

1859 Shipton, William Parker, Consulting Surgeon to the Devonshire Hospital; Buxton, Derbyshire.

1861 Shortt, John, M.D., Surgeon H.M. Madras Army, and Superintendent-General of Vaccination, Madras Presidency. *Hon. Loc. Sec.* [Agents: Messrs. Baring, Brothers, 8, Bishopsgate within, E.C.]

1874 Simpson, George Alex., M.D., Hampstead lane, Highgate, N.

1874 Sinclair, Alexander Doull, M.D., late Physician to the Boston City Hospital; 35, Newbury street, Boston, Massachusetts, U.S.

1874 Skinner, Stephen, M.B., Edgecliffe, Clevedon, Somerset.

1874 Slayter, W. B., M.D., Halifax, Nova Scotia.

1861 Sloman, Samuel George, Farnham, Surrey.

Elected

1861 Slyman, William Daniel, 26, Caversham road, Kentish Town, N.W.

1860 Smart, Thomas Tovey, L.R.C.P. Ed., South Villa, Bedminster, Bristol.

1859 Smiles, William, M.D., Surgeon to the House of Correction, Cold Bath Fields; 44, Bedford square, W.C.

1867 Smith, Heywood, M.D., Physician to the Hospital for Women, Soho square, and Physician to the British Lying-in Hospital; 2, Portugal street, Grosvenor square, W. *Council*, 1872-5.

1873 Smith, Joseph, 43, David place, St. Helier's, Jersey.

O.F. Smith, Protheroe, M.D., Physician to the Hospital for Women, Soho square; 42, Park street, Grosvenor square, W.

1875 Smith, Richard Thomas, M.D., Assistant-Physician to the Hospital for Women, Soho square; 21, Haverstock hill, N.W.

1875 Smith, Ernest Louis Tyler, M.A., M.B. Cant., Hounslow.

1859 Smith, William Johnson, M.D., Consulting Physician to the Weymouth Infirmary and Dispensary; Greenhill, Weymouth, Dorset. *Council*, 1869-71.

1866 Soper, William, Medical Officer, Jews' Hospital, Norwood 4, Clapham rise [283, Clapham road], S.W.

1869 Spaull, Barnard, F.R.C.S., Essex House, Vale place, Hammersmith, W.

1868 Spaull, Barnard E., 2, Vale place, Hammersmith, W.

1872 Spence, James Beveridge, M.D., Earlswood Asylum, Red Hill, Surrey.

1859 Spencer, George, 8, Kensington park road, W.

1862 Spry, G. Frederick Hume, M.D., Assistant-Surgeon, 2nd Life Guards, Army and Navy Club, S.W.

O.F. Squire, William, M.D., M.R.C.P., 6, Orchard street, Portman square, W. *Council*, 1866-68. *Vice-Pres.* 1876.

1866 Steele, Arthur Browne, L.K.Q.C.P. Ireland, Lecturer on Midwifery, Royal Infirmary School of Medicine; 54, Rodney street, Liverpool. *Council*, 1874-6.

Elected

1873 Steele, Edward Harry, Kingston-on-Thames.

1873 Stewart, James, M.D., 2, Skinner street, Whitby, Yorkshire.

1875* Stewart, William, L.R.C.P. Ed., Highfield House, Barnsley, Yorkshire.

1859 Stone, Joseph, M.D., 175, Upper Brook street, Manchester.

O.F. Stowers, Nowell, 125, Kennington park road, Kennington, S.E.

1875 Strange, Frederic William, Aurora, Ontario.

1866 Strange, William Heath, M.D., 2, Belsize avenue, Belsize park, N.W.

1871 Sturges, Montague J., M.D., Elmstone House, Beckenham, Kent.

1859 Stutter, Frederick Augustus, M.D., Farnboro' House, Upper Sydenham, S.E.

1862 Sutton, Field Flowers, M.D., Balham hill, Clapham, S.W.

1859 Swayne, Joseph Griffiths, M.D., Physician-Accoucheur to the Bristol General Hospital; Harewood House, 74, Pembroke road, Clifton, Bristol. *Council*, 1860-61, *Vice-Pres.* 1862-64. *Hon. Loc. Sec.*

1850 Sweeting, George Bacon, M.R.C.P., King's Lynn, Norfolk.

1859 Symonds, Frederick, M.A., F.R.C.S., Surgeon to the Radcliffe Infirmary; 35, Beaumont Street, Oxford. *Council*, 1862-65. *Hon. Loc. Sec.*

1872 Szczygielski, Joseph, M.D., St. Terska, No. 24, Warsaw, Russian Poland.

1871 Tait, Lawson, F.R.C.S. Ed. and Eng., L.R.C.P. Ed.; Surgeon to the Birmingham and Midland Hospital for Women, and to the Lying-in Charity; Consulting Surgeon to the Church Stretton Asylum for Ladies; 7, Great Charles street, Birmingham.

1866 Tannahill, Robert Dunlop, M.D., Physician to the Lying-in Hospital; 106, Bath street, Glasgow.

Elected

1871 TANNER, JOHN, M.D., F.L.S., Physician for Diseases of Women, to the Farringdon General Dispensary, and Obstetric Physician to the Lying-in Charity, Holborn; Alfred House, Newington Causeway, S.E.

1859 TAPSON, ALFRED JOSEPH, M.B. Lond., 36, Gloucester gardens, Westbourne terrace, W. *Council*, 1862-64.

1863 TAPSON, JOSEPH ALFRED, Surgeon to the Clapham General Dispensary; 83, High street, Clapham, S.W.

1871 TAYLER, FRANCIS F., B.A. Lond., and M.B., Claremont villa, 224, Lewisham road, S.E.

O.F. TAYLOE, EDWARD, South lodge, Clapham common, S.W.

1870 TAYLOR, ARTHUR, M.B., 180, Kennington park road, S.E. [Marton, Province of Wellington, New Zealand.]

O.F. TAYLOR, CHARLES, M.D., Pine house, Camberwell green, S.E. *Council*, 1869-71.

1859 TAYLOR, DAVID, 180, Kennington park road, S.E.

1869 TAYLOR, JOHN, Earl's Colne, Halstead, Essex.

1871 TAYLOR, JOHN W., M.D., 34, Queen street, Scarborough.

1862 TAYLOR, THOMAS, F.R.C.S., 19, Bennett's hill, Birmingham. *Council*, 1875-6.

1872 TEMPLE, JAMES ALGERNON, M.D., Lecturer on Midwifery, Trinity College, Toronto.

1862 THANE, GEORGE DANCER, M.D., 15, Montague street, Russell square, W.C.

1872 THOMAS, JAMES BYER, Assistant-Surgeon H.M.'s Indian Army, Madras and Zillah Surgeon, Tinnevelly.

1870 THOMPSON, JOHN ASHBURTON, 207, Caledonian road, N.

1867 THOMPSON, JOSEPH, Junr., 26, Regent street, Nottingham.

1869 THOMPSON, D. R., M.D., Officiating Civil Surgeon, 26th Native Infantry, Trichinopoly. [Messrs. de Beaux & Co., Esplanade Dispensary, Black Town, Madras.]

1874 THOMSON, WILLIAM, M.D., Consulting Surgeon, Peterborough Infirmary, Priestgate street, Peterborough.

Elected

1867 THORBURN, JOHN, M.D., Lecturer on Midwifery, Manchester Royal School of Medicine; 333, Brighton place, Oxford street, Manchester. *Council*, 1876.

1860 THORNE, GEORGE LEWORTHY, M.D., Swanage, Dorset.

1871 THORNE, WILLIAM BEZLY, 46, Harcourt terrace, Redcliffe square, South Kensington, S.W.

1862 THORNTON, WILLIAM HENRY, M.D., Springfield terrace, Dewsbury, Yorkshire.

1867 THORNTON, WILLIAM HENRY, Surgeon to the Royal National Hospital for Scrofula; Berkeley Lodge, Margate.

1874 TICEHURST, AUGUSTUS ROWLAND, Silchester House, Pevensey Road, St. Leonard's-on-Sea.

1873 TICEHURST, CHARLES SAGE, Bishop's Waltham, Hants.

1860 TIFFEN, ROBERT, M.D., Wigton, Cumberland.

1866 TILLEY, SAMUEL, 70, Union road, Rotherhithe, S.E.

O.F. TILT, EDWARD JOHN, M.D., Consulting Physician-Accoucheur to the Farringdon General Dispensary; 27, Seymour street, Portman square, W. *Council*, 1867-68, 1875-6. *Vice-Pres.* 1869-70. *Treas.* 1871-2. *Pres.* 1873-4.

O.F. TIMES, HENRY G., 23, Manchester street, Manchester square, W.

1872 TOLOTSCHINOFF, N., M.D., Kieff, Russia [*per* M. N. Orloff, 44, Lupus street, Pimlico].

1869 TOMKINS, CHARLES P., L.K.Q.C.P.I., Beddington park, Croydon.

1870 TOWNE, ALEXANDER, Junr., 364, Kingsland road, N.E.

1872 TRENHOLME, EDWARD HENRY, M.A., M.D., C.M., Professor of Midwifery, University of Bishop's College, Montreal, Canada.

1873 TRESTRAIL, HENRY ERNEST, F.R.C.S., L.R.C.P. Ed., 6, Trinity terrace, Victoria road, Aldershot.

1872 TUCHMANN, MARO, M.D., 148, Adelaide road, Haverstock hill, N.W.

Elected

1865 Turner, John Sidney, Surgeon to the Anerley Dispensary, Stanton House, Thicket road, Upper Norwood, Surrey.

1861 Turner, Richard, Surgeon to the Lewes Dispensary; High street, Lewes, Sussex.

1871 Turton, Frederic, Ablow House, Wolverhampton.

1861 Tweed, John James, Junr., F.R.C.S., 14, Upper Brook street, W.

1874 Underhill, Thomas, M.D., Summerfield, West Bromwich, Staffordshire.

1860 Varenne, Ezekiel G., Kelvedon, Essex.

1874 Venn, Albert John, M.B., 30, New Cavendish street, W.

1873 Verley, Reginald L., M.R.C.P. Ed., 88, Gower street, W.C.

1864 Wahltuch, Adolphe, M.D., 280, Oxford street, Manchester.

1860 Wales, Thomas Garneys, Downham Market, Norfolk.

1869 Walker, Alfred, M.D., late Physician to the East London Hospital for Sick Children; Fore street, Hertford. *Hon. Loc. Sec.*

1866 Walker, Thomas James, M.D., Surgeon to the General Infirmary, Peterborough; 18, Westgate, Peterborough. *Hon. Loc. Sec.*

1873 Walker, Thomas Osborne, Crick, near Rugby, Northamptonshire.

1870 Wallace, Frederick, District Surgeon to the Royal Maternity Charity; 243, Hackney road, N.E.

1872 Wallace, John, M.D., Assistant-Physician to the Liverpool Lying-in Hospital; 4, Canning street, Liverpool. *Hon. Loc. Sec.*

1872 Waller, Charles Beaumont, Sydenham, S.E.

1860 Waller, John Turpin, Flegg Burgh, Norfolk.

1875 Waller, Walter Kerr, Kyd street, Calcutta.

1867 Walters, James Hopkins, Faringdon, Berks.

1873 Walters, John, M.B., Bell street, Reigate, Surrey.

O.F. Wane, Daniel, M.D., 20, Grafton street, Bond street, W.

Elected

1859 WARDEN, CHARLES, M.D., Hon. Surgeon to the Birmingham Lying-in Hospital; 39, Temple street, Birmingham.

1874 WARN, REUBEN THOMAS, 37, Highgate road, N.W.

1862 WATKINS, CHARLES STEWART, 16, King William street, Strand, W.C.

1861 WATTS, GEORGE HENRY, Thatcham, near Newbury, Berks.

1873 WAY, JOHN, M.D., 4, Eaton square, S.W.

1867 WEBB, FRED. E., 113, Maida vale, W.

O.F. WEBB, HENRY SPEAKMAN, Welwyn, Herts.

1872 WEBSTER, THOMAS, Malvern House, Redland, near Bristol.

1860 WELCHMAN, CHARLES EDWARD ELLIOT, Bore street, Lichfield, Staffordshire.

1867 WELLER, GEORGE, Forest lodge, Wanstead, Essex.

1874 WELLS, HARRY, M.D., H.B.M. Vice-Consul, Gualeguaychu, Entre Rios, Argentine Confederation.

O.F. WELLS, T. SPENCER, F.R.C.S., Surgeon in Ordinary to H.M.'s Household; Surgeon to the Samaritan Free Hospital for Women and Children; 3, Upper Grosvenor street, W. *Council*, 1859. *Vice-Pres.* 1868-70. *Trustee.*

1859 WESTMACOTT, JOHN GUISE, M.D., Medical Officer to the Paddington Provident Dispensary; 19, St. Mary's terrace, Paddington, W.

1870 WHEATCROFT, SAMUEL HANSOM, L.R.C.P. Ed., Stoke Ferry, Brandon, Norfolk.

1860 WHEELER, DANIEL, Chelmsford, Essex.

1873 WHITE, FREDERICK BROAD, 15, Maida vale, W.

1860 WHITE, FREDERICK GEORGE, L.R.C.P. Ed., Castle House, Chepstow, Monmouthshire.

O.F. WHITEHEAD, JAMES, M.D., Physician to the Manchester Clinical Hospital; 87, Mosley street, Manchester. *Council*, 1859-61. *Vice-Pres.* 1868-69.

1864 WHITMARSH, WILLIAM MICHAEL, M.D., M.R.C.P. Ed., Surgeon to the Hounslow Lying-in Charity: Albemarle House, Hounslow, Middlesex.

Elected

1867 WILBE, RICHARD HAYDOCK, M.D., York Lodge, 21, Finchley road, St. John's Wood, N.W.

1873 WILKINS, RICHARD, Hon. Assistant Surgeon, Madras Army, Demonstrator of Anatomy in the Medical College, Madras; Madras.

1871 WILKINSON, WILLIAM HENRY WHITEWAY, L.R.C.P. Ed., 268, Caledonian Road, Islington, N.

1861 WILLIAMS, ARTHUR WYNN, M.D., Physician to the Samaritan Free Hospital; 1, Montagu square, W. *Council*, 1871.

1864 WILLIAMS, EDWARD, M.D., Holt street House, Wrexham.

1867 WILLIAMS, HENRY LLEWELLYN, M.D., 9, Leonard place, Kensington, W.

1872 WILLIAMS, JOHN, M.D., Assistant-Obstetric Physician to University College Hospital; 28, Harley street, Cavendish square, W. *Council*, 1875-76.

1860 WILLIAMS, ROBERT HANKINSON, L.R.C.P. Ed., Great Eccleston, near Garstang, Lancashire.

1873 WILMOT, ROBERT EARDLEY-, M.B., Ottery St. Mary, Devon.

1859 WILSON, JAMES GEORGE, M.D., Professor of Midwifery in the Andersonian University, Glasgow; Consulting Physician-Accoucheur to the Glasgow Maternity Hospital; 9, Woodside crescent, Glasgow. *Council*, 1863-64. *Vice-Pres.* 1865-67.

1873 WILSON, JOHN HENRY, L.K.Q.C.P. Ireland, Obstetric Physician to the Ladies' Charity and Lying-in Hospital, Kensington Lodge, Kensington, Liverpool.

1860 WILSON, ROBERT JAMES, F.R.C.P. Ed., 7, Warrior square, St. Leonard's-on-Sea, Sussex. *Hon. Loc. Sec.*

1868 WILTON, JOHN, L.R.C.P. Ed., Chalk Pit House, Sutton, Surrey.

1866 WILTSHIRE, ALFRED, M.D., Assistant-Obstetric Physician to St. Mary's Hospital, and Physician for the Diseases of Women to the West London Hospital; Physician to the British Lying-in Hospital; 57, Wimpole street, Cavendish square, W. *Council*, 1870. *Hon. Lib.* 1871-3. *Hon. Sec.* 1874-6.

Elected

1872 Wise, William Clunie, M.D., Gothic villa, Burrage road, Plumstead, S.E.

1860 Wiseman, William Wood, Springstone House, Ossett, near Wakefield, Yorkshire.

1872 Wood, Robert Arthur Henry, 102, Pembroke place, Liverpool.

1860 Wood, William James, Brightwaltham, Wantage, Berks.

1864 Woodman, William Bathurst, M.D., Assistant-Physician to the London Hospital; 6, Christopher street, Finsbury square, E.C.

O.F. Worship, J. Lucas, Manor House, Riverhead, Sevenoaks, Kent. *Council*, 1875-76.

1871 Yarrow, George Eugene, M.D., 87, Old street, E.C.

1866 Yeaman, George, M.D., 5, Windsor place, Sauchiehall street, Glasgow.

1870 Yeates, George, M.D., Grove road, Walthamstow, Essex.

1874 Young, David, M D., 13, Via dei Fossi, Florence, Italy.

1861 Young, William Butler, 10, Castle street, Reading, Berks.

1869 Yule, John S. C., Castlefield House, 78, Walmsey road, Bury, Lancashire.

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The Society is not as a body responsible for the facts and opinions which are advanced in the following papers and communications read, or for those contained in the abstracts of the discussions which have occurred at the meetings during the Session.

OBSTETRICAL SOCIETY

OF

LONDON.

SESSION 1875.

ANNUAL GENERAL MEETING.

JANUARY 6TH, 1875.

EDWARD JOHN TILT, M.D., President, in the Chair.

Present—50 Fellows and 3 visitors.

The Ballot for the Election of Officers and Council for 1875 was declared open till 9 o'clock, and Dr. Hayes and Dr. Roper were appointed Scrutineers.

Books were presented by Dr. Faye, Dr. Liebman, Prof. Rizzoli, Dr. Spiegelberg, and Dr. M. Tuchmann, the staff of St. Bartholomew's Hospital, and the Philadelphia Obstetrical Society.

Mr. Alex. Fergusson, Dr. R. A. Douglas Lithgow, and Mr. Wm. Rees, were admitted Fellows of the Society.

Dr. C. R. Brown and Mr. Aug. R. Ticehurst were declared admitted.

The following gentlemen were elected Fellows of the Society: Alex. Hampton Brewer, M.R.C.S.; Angus Fraser, M.D. (Aberdeen); William Owen, M.R.C.S. (Ebbw Vale); and Walter Alfred Satchell, M.R.C.P. Ed. (Kew).

Dr. A. Cordes (Geneva), Mr. R. J. Morton (Aylsham), D. Ernest W. Paul, and Dr. Rd. Tho. Smith, were proposed for election.

Professor Gustav Simon, of Heidelberg, and Professor Courty, of Montpellier, were recommended by the Council for election as Hon. Fellows of the Society.

CASE OF HYDATIDIFORM MOLE.

By John Williams, M.D.

C. D—, æt. 30, a cook, was admitted into University College Hospital October 26th, 1874. Married two years, but had never been pregnant before.

Catamenia appeared once when she was twelve years of age, but then ceased until her seventeenth year. She had severe pain in the left ovarian region, back, and thighs. Just before and during the period so severe was the pain that she was frequently confined to her bed by it. She suffered also from vomiting during the period. Periods were regular, and lasted four days.

Since her marriage the discharge has contained clots; she has had frequent fits of hysteria.

Her last period took place on June 28th, 1874; shortly after she suffered from morning sickness and occasional sickness after food.

During the month of August she was engaged in nursing an invalid, and had frequently to exert herself in

lifting the patient; especially did she strain on 17th August. After this the sickness became worse, and she soon noticed that her feet and ankles were swollen. The œdema gradually progressed upwards until the lower part of the abdomen and the labia (which were enormously swollen) became affected.

On October 17th the labia were incised, and a large amount of fluid continued to ooze out of them for several days. There was no œdema of face or upper limbs.

The abdomen was symmetrically enlarged. Umbilicus slightly prominent; veins on surface large. A tumour having the characters of the distended uterus was felt in the abdomen. It was soft and elastic, but not fluctuating. No fœtal heart sounds were audible, but a bruit similar to the placental bruit was once heard on left side a little below the umbilicus.

The tumour could be felt on examination per vaginam filling the pelvis; the os uteri was high up and tilted forward close to the pubes, and it was not characteristic of pregnancy. The tumour was softish, as if it contained fluid, and a smaller triangular swelling filled the hollow of the sacrum. Taken together they had the characters of a retroverted gravid uterus. The breasts were enlarged and the areolæ darker than usual, but were not characteristic of pregnancy. A small quantity of watery fluid could be pressed out of the nipple.

On the night of Nov. 10th she was taken in labour, and was delivered on the following morning of an hydatid mole. After labour had set in the cervix of the uterus came to occupy the centre of the pelvis.

The mass expelled consisted of a ruptured bag having the shape of the uterus, and which contained the vesicles forming the mole. The bag was evidently formed by the decidua, and was about a quarter of an inch in thickness. At one part it was thick and fleshy, though on section it presented many cysts. This thickened portion was doubtless the imperfectly formed placenta.

Slight hæmorrhage from the vagina occurred once,

some days before the onset of labour; when labour set in profuse bleeding took place, and it was found on examination that the placenta was prævia; this was proved not only by the finger, but by observation of the part which passed first through the vulva.

The case is unusually interesting, as the decidua was expelled almost in its entirety, and thus the mode of attachment of the vesicles to that structure was shown. They were arranged like beads on a string, which string was attached to the inner surface of the decidua. This fact favours the view that the hydatidiform mole is formed by degeneration of the chorion villi, and in this case the degeneration commenced probably soon after the formation of the placenta had began.

Dr. Routh thought it was important to hear the history. He would be glad to know whether there were any special symptoms —any alternation of sanguineous and watery discharge.

COMPLICATION IN THE DELIVERY OF AN ASCITIC FŒTUS; THE SPECIMEN EXHIBITED.

By J. Ashburton Thompson,

FELLOW, SURGEON-ACCOUCHEUR TO THE ROYAL MATERNITY CHARITY.

As Dr. J. Hall-Davis' deputy in this neighbourhood I was summoned to the following case, which occurred in his district of the Royal Maternity Charity.

E. W—, æt. 38. Tenth confinement. Had been in labour twenty-three hours. The first stage had been completed about eight hours. Advice was sought on account of inefficient uterine contraction. On examination I found the head presenting, in the cavity of the

pelvis. The patient believed herself to be six weeks short of her time, and she had not felt the child for a fortnight. The head was plainly that of a child not arrived at maturity. Ergot had been given without effect. Under these circumstances I apprehended no difficulty in extracting the fœtus with the forceps, which I accordingly applied, and with very little force, chiefly by extending the head without direct traction, the latter was easily delivered. Pain not being excited by this proceeding, slight pressure was made on the abdomen, which I then discovered to be rather larger than usual. On making further traction on the head I felt the neck giving way; and on examination found it had been delivered entirely at the expense of the tissues of the neck, which were torn almost completely through; the shoulders still occupied the same position as before interference. I then brought the arms down and found that they would sustain considerable force without yielding; and through them I was able to exert as much traction as was advisable. Nevertheless, I was unable to alter the position of the body of the fœtus—it perfectly resisted every effort at delivery, continued during twenty minutes. Examination of the abdomen showed that there was but one fœtus within it. Suspecting some deformity of the child I thought well to seek advice of Dr. Brunton, in whose immediate neighbourhood the case occurred. But on returning after a short absence I found that two pains, said by the midwife to be trifling pains, had sufficed to expel the body. The placenta was pale and very friable, and, after every other means had been tried to extract it, it was necesssary to remove it by hand. This was done completely, though with some difficulty, as it was adherent in many places. There was no hæmorrhage.

Twelve hours from delivery the mother was doing well; in fact, sitting up in bed and eating her dinner. The discharge had been normal in quantity and quality. Had passed water.

Sixty hours.—She began to get a little feverish, and

to complain of pain in the left shoulder and right wrist-joint. The bowels had acted without aperient.

Sixty-seven hours. — Pulse 130 ; temperature 105° (axilla). Tongue dry and getting furred. The pains complained of are excruciating. The left shoulder is now swelled uniformly over the joint ; but the pain in the wrist is associated with swelling, not in the joint but a little above it, and over the anterior surface of the ulna. It presents the appearances of inflammation, but there is no fluctuation. Is talkative, but rational. Lochia quite normal. To take four grains of quinine and one grain of opium every four hours. Six ounces of brandy, beef-tea, &c.

Eighty-four hours.—Pulse 136 ; temperature 104·5° (axilla). Tongue furred, dry, and brown. The swelling above the wrist has quite disappeared ; that in the shoulder is reduced, and there is now neither pain nor tenderness. Lochia normal ; especially, not offensive. Has taken beef tea, brandy, &c., well. Has been wandering all day, but answers rationally. No cinchonism. To continue.

Ninety-three hours.—Having been rather quieter and more rational during the afternoon of this day, at the ninety-third hour she raised herself up in bed, fell back, and died in a few minutes. There was no fresh symptom.

Remarks.—The delivery of this case offers two points for remark. This patient was a half-fed, half-clothed, miserably-debilitated woman, in her tenth confinement at thirty eight years of age ; and in these circumstances may, perhaps, be seen reason enough for the uterine inertia which is reported. But it is not only in such women, on the one hand, nor even invariably in such women, that inertia is witnessed ; while, on the other, it is a constant result of over-distension of the uterus, whether from abnormality of the fœtus or its excessive development, from a superabundance of the *liquor amnii* or from the presence of twins. Theory would anticipate

this, and common observation corroborates it; nor would I draw your attention to so trite a remark but that it seems to me to afford ground for a practical suggestion. It is to be deplored, though possibly the issue of the case was not affected by the delay, that advice was not sooner sought; and it would be well, having warned inexperienced attendants on midwifery of this cause of inertia, to instruct them to ask assistance whenever the first stage of labour has been completed two or three hours, the presentation and capacity of the pelvis being normal, and the only apparent cause of delay being lack of expulsive pain. If it be suspected that the uterus is of unusual size that would be another reason for avoiding delay.

Secondly, why was a trifling amount of force applied in one direction sufficient to effect what very considerable force applied in the opposite direction failed to effect? The answer to this question resolves itself, I believe, simply into this: that pressure being applied to the upper part of the body of an ascitic fœtus the cavities are compressed and the fluid compelled to occupy the smallest section of the abdomen, at the expense of increasing its circumference to the greatest possible extent; but pressure applied by the uterus—that is, to the lower parts of the fœtus—would have for its very first effect the passage through the brim of the maternal pelvis of a part of the fluid; the whole of the abdominal cavity of the fœtus, and no doubt in part the thorax too, would be encroached upon; and thus the greatest amount of space would become available for spreading out the column of fluid, and in consequence the circumference of the fœtal body would be by so much reduced; hence the ease with which delivery was effected by the uterus itself. This is a complication of which I have been unable to find any mention. Should it occur again this case suggests the propriety of giving a careful trial to pressure over the uterus before proceeding to any more serious operation.

With regard to the ultimate fate of the mother, there is scarcely room for doubt that she died of pyæmia, short

as the interval between delivery and death was; and the question arises, was the condition of pyæmia existent before delivery? On this point I can offer no suggestion; beyond the facts of poverty and debility already mentioned, there was nothing to attract attention to her condition twelve hours after delivery. I did not then take her temperature, it is true; but with that exception I made a close examination of her, and can say that she presented no symptom. I cannot think either that any lung complication hastened her death. She was confined on the morning of the new year, and the night as well as the previous day had been miserably cold. During all this time she had lain only partially covered, and in such a subject it might be surmised that an acute disorder of the lungs had supervened; but there was no symptom referable to them present. On the other hand, the fœtus had been dead a fortnight or longer, and was much decomposed. A post-mortem examination of the mother was not allowed.

Dr. Barnes inquired whether the placenta was pale and friable.

Mr. Thompson replied in the affirmative.

Dr. J. Williams suggested, that as the specimen was not a common one it should be further examined.

Dr. Williams and Dr. Hayes, in conjunction with Mr. Thompson, were requested by the President to examine and report upon the specimen.

ON THE PREVENTION OF MAMMARY ABSCESSES BY THE APPLICATION OF THE PRINCIPLE OF REST.

By W. Bathurst Woodman, M.D.,

ASSISTANT PHYSICIAN TO THE LONDON HOSPITAL, AND PHYSICIAN TO THE NORTH-EASTERN HOSPITAL FOR CHILDREN.

I must premise the remarks I have to offer on this subject by stating that the ideas contained in this paper were suggested to my mind long before Mr. Hilton's celebrated lectures "On Pain and the influence of Physiological and Mechanical Rest in the treatment of Surgical Diseases" were published or even written. It was during my apprenticeship, and whilst living in the country, that I was struck with the rarity of mammary abscess in animals, notwithstanding the forced abstinence from suckling which cats and dogs undergo from the drowning of their progeny; and in spite of the great distension of the udders of cows, mares, and other animals, when driven to market, or for other reasons separated from their young. It then occurred to me that if the principle "ubi stimulus, ibi fluxus" were true, its converse need not necessarily be false; and that if the natural powers sufficed for the dessication or absorption of the milk already secreted, and to procure what I may call resolution of the threatened inflammatory processes in the mammary glands of the lower animals, the same powers would, in all probability, avail for the same ends in the human female. I resolved, therefore, in the next case of threatened mammary abscess from over-distension which should come under my care, to utterly forbid those manipulations and those questionably "gentle" frictions which have so long been customary in such cases. I soon had opportunities of applying my experience of comparative pathology to both primiparæ and pluriparæ who had given birth to stillborn infants. This principle of rest has now proved

so successful in my hands that I wish now, not to claim any priority for either the principle or the practice, for it may be that some of our older fellows have applied this very principle for many years; but to bring it prominently before the notice of the Society, and to point out how almost uniformly successful it is in the following cases of threatened abscess in the female breast.

1st. After the birth of a dead child at or about the full term of gestation. In such a case the breasts often become very speedily swollen, often to an enormous size; throbbing, tense, tender, and heavy, and at last agonizingly painful; the milk perhaps running away from the nipples all the while.

2nd. In cases where the infant may survive its birth only a few hours or days, and a similar condition of breasts to that described soon becomes set up.

3rdly. In cases where, either from the child sucking imperfectly, or from its mouth being aphthous, or from some previous condition of the nipples, such as imperfect development, these become sore, and thus mammary abscess becomes imminent; partly from the great distension of the breasts, and partly from contiguity of tissue; and—

4thly. In all cases of threatened abscess of the breast, no matter what their ætiology, where we have this painful condition of turgescence; but without any of those positive signs of fluctuation, or of the occurrence of suppuration which would justify our having recourse to the knife.

In many of these cases, whatever the cause of the mammary distension, and of the threatened "milk abscess," there is so general a consensus amongst surgeons and obstetricians in the recommendation of "gentle frictions," and of the use of liniments in the way of "illination," that it seems superfluous for me to take up the time of the Society with a long parade of quotations which must necessarily partake of sameness. The late Dr. Tyler Smith attempted to explain the real or supposed beneficial effects of frictions, by referring to the lymphatics of the female breast; of

which he says: "It is to these vessels that we address ourselves when we endeavour to diminish the quantity of the milk by frictions" ('Manual of Obstetrics,' p. 30). I do not wish to be misunderstood, and will therefore say at once that I do not dispute that there are cases, particularly where the mischief we dread is yet young, in which very great and evident relief may be obtained by really gentle frictions, judiciously and tenderly applied. No doubt in this, as in many other departments of our art, it is true that "medium tenuere beati;" but knowing as I do, and as hundreds of poor women know by sad experience, how difficult it is to meet with those skilled in such gentle manipulations, and how seldom our advice is asked till the mischief has advanced so much that every touch only adds fuel to the fire of the commencing inflammation, I feel that we can add this satiric comment to the friction treatment, "Plerique sunt damnati"—"Very many of these cases are lost"; that is to say, that the symmetry and the usefulness of one of the chief elements of female beauty are too often lost, and lost irretrievably, by the rough handling to which these organs have been subjected by well-meaning nurses, acting under medical orders. Let the surgeon once order a liniment, and the female enthusiast who is to execute his orders too often applies a degree of potential energy which would be well enough if applied to the scouring of a stone floor; but is painfully out of place in a delicate tissue like that of the breast, already predisposed to inflammation and its sequelæ.

Before mentioning any illustrative cases by way of a conclusion to these hints, I shall briefly inquire into some of the ways in which rest may be applied to the treatment of incipient inflammation of the female breast.

1st. It has already been said that the avoidance of frictions and of all rough handling is considered by me a sine quâ non in the majority of such cases. Case No. 1 illustrates this.

2nd. Suckling must be avoided for a time, if possible,

from both breasts, but at all events from the one most implicated. There need be no fear of the infant altogether losing its sustenance, for when the breast or breasts have recovered it is usually not difficult by simple means to induce a fresh and sufficient secretion of milk.

3rd. For the same reason the milk should not be drawn off by the breast-pump, or, at all events, not more than once, or at the most twice, at a considerable interval, since this is but an artificial suckling.

4th. The horizontal position, rest in bed or on a couch, will greatly diminish the heavy "drag" of the over-loaded breasts.

5th. Careful application of strips of "isinglass," "soap," or "lead" plaster, or of an air-cushion with a hole in its centre, or of bandages taking their purchase, so to speak, from the opposite shoulder, are subsidiary means of giving rest locally, and are too well known to need description.

6th. Preparations of opium, belladonna, or chloroform applied on compresses (not rubbed in); or even in some instances the use of ice, of moist warmth, or even leeches; or of a continuous current of iced or tepid water through caoutchouc tubes, may all assist in giving rest to the nerves of the part or to the blood-vessels.

7th. Relieving the local congestion by diaphoretics such as salines and antimony, or by diuretics and mild purgatives, may also conduce to the same end.

8th. Medicines such as belladonna, the iodide of potassium, or any other of the so-called lactifuges, which may lessen the secretion of milk, would clearly come under our principle.

Lastly, medicines which procure sleep, and even tonics, and all the means which skill or affection may suggest to divert the mind of the sufferer from the local affection, would all tend to the general end of local rest.

I will conclude these brief hints by a short narration of one or two striking cases out of some dozens, at least, of cases treated on these principles, in not one of which did the threatened and dreaded abscess supervene in spite of

the enormous and painful distension of the breasts, which caused us to fear such a termination.

Case 1.—*Swelling of the breasts in a primipara after stillbirth.*—Mrs. C—, aged 23, fair, stout, and very full-blooded, gave birth to a deadborn child, of full size and at full term, after a rather lingering labour. She ascribed the death of the child to the great anxiety, fatigue, and fright she had undergone a very short time before the labour. Her breasts had always been large, and the nipples were badly developed. Immediately after delivery they swelled to an enormous size, were most tender and painful, and milk ran away. Her husband, who was a medical man, adopted my views, and persuaded her not to have any frictions, and not to use the breast-pump. Saline purgatives, rest in bed, and the use of opiates to relieve pain, soon made her fairly comfortable; but the breasts remained of enormous size for nearly fourteen days, so that she was much afraid that there would be permanent disfigurement. Her husband now begged me to allow the breast-pump, or, at least, that an infant or grown-up person who could suck vigorously should be allowed so to do. I begged them to have patience, and to be satisfied with a local application of belladonna and a little mechanical support. The lady submitted to this, and the end justified the means, or rather our faith in natural methods, for in a few days more the milk was all dried up or absorbed, and the breasts resumed their natural size and appearance.

Case 2.—A lady who had had several children suddenly lost her last infant at the early age of seventeen days from convulsions, the exact cause of which it was difficult to trace. The distension of the breasts in this case was very painful, even to agony, as she expressed it. She had besides some shiverings (probably due to pelvic causes) which made us, however, very apprehensive of abscess in one if not both breasts. In this case these symptoms were pretty quickly relieved by full doses of opium with anti-

mony; and by carefully strapping the breasts and then suspending them by broad bandages from the corresponding shoulders.

Case 3.—A primipara, æt. 19, after suckling her child a few days, complained of sore nipples, which her enlightened nurse said "were a good sign." Neglect of any treatment soon caused suckling to be so painful that she speedily discontinued it; the breast got worse and worse, and when I saw her I had little hope of preventing abscess in the right mamma, the nipple of which was ulcerated, and had always been imperfectly formed. Glycerine of tannin was applied to the nipples, and iced and warm water were applied by turns for a couple of hours for each method, by means of a circular cushion of india-rubber with a hollow centre. Ten-grain doses of iodide of potassium were also given three times daily, and an opiate the first night, after which it was no longer necessary. These means soon reduced the size of the breasts, and checked the morbid symptoms; after a few days she was quite free from all pain. Being really desirous of suckling her child, she was now frightened lest she should never get her milk again, but by the aid of a breast-pump to draw out the nipples, and warm fomentations, together with the regular and systematic application of the child, a proper "shield" being now used, she soon had the gratification of finding "the flow of milk" re-established, and her child fortunately suffered little from its enforced privation, whilst she herself escaped the horrors of "a bad breast" which might have been protracted through many weeks of agony.

In a Society where the majority of members are daily meeting with cases of the kind I have described, it would be superfluous to heap up illustrative cases. I trust enough has been said to induce others thus to follow the indications furnished us by nature. I feel sure that the less we narrow our pathology or our therapeutics to the

lessons drawn from mere observation of human beings, and the more we study the progress of disease and the processes of cure in the domains of comparative pathology and of animal therapeutics in the widest sense, the more successful and the more certain will be our practice amongst members of the human family.

Dr. Barnes observed that the principle of rest had long been applied to the treatment of inflammation of the breast. He himself had learned the value of it from Trousseau when a student in Paris thirty years ago. That admirable physician taught and illustrated it with great earnestness. He placed the breast at perfect rest by carrying straps of leather spread with *Emplâtre de Vigo* all round it, so as to lift it well up, and exert constant support on the vessels. Thus œdema was prevented and engorgement soon subsided. It must, however, be remembered that this form of pressure was ill-borne in the first inflammatory stage; it was chiefly serviceable when suppuration had taken place if the abscess had been opened. The sac was then rapidly closed. In the earlier stages he had seen leeches do excellent service. The pressure then must be lighter.

Mr. Ashburton Thompson said: There are two modes of treatment to which I did not hear Dr. Woodman refer—by the administration of the tincture of aconite and by total abstention from fluids during the necessary number of days. By giving minim doses of the pharmacopœial tincture of aconite every hour, I have been successful in cutting short inflammations of the breast, which, there was no doubt, would otherwise have run on to suppuration, very frequently indeed in three cases out of five. For the suppression of the secretion of the breast in cases of stillbirths I have hitherto found abstention from fluids sufficient in every case to avoid every kind of mammary disturbance. Ice is allowed in moderate quantity and no other fluid from the time of delivery until the fourth or fifth day, when the breasts generally return to their normal state of quiescence. I can illustrate this by reference to two cases which have occurred in my practice during the last three weeks. Each patient was a primipara aged 30; and each was a stout woman with very well-developed mammæ. One was delivered by craniotomy; in the other, a breech presentation in the vaso-posterior position, coincided with a narrow pelvis, and the child was extracted, still with much trouble. Both of these patients were treated by abstention from fluids, a little ice only being allowed. In one the breasts appeared to subside straightway from delivery; in the other there was a little delay in subsiding; but both patients

were able to live as usual at the end of six days without experiencing the least inconvenience. The deprivation from fluid causes but little distress.

Dr. Braxton Hicks remarked that the principle was gradually coming upon us for years. Friction was only employed among the poor and uneducated. The experience of surgery was all tending to quietude, manipulation tended to produce suppuration, the extra amount of stimulation being just sufficient to set it up.

Dr. Murray thought the application of a belladonna plaster and keeping the arms fastened to the side were the best measures to adopt. He thought slight friction upwards productive of good.

Dr. Matthews, while heartily assenting to Dr. Woodman's views, thought that the public also had largely endorsed his practice, since he had observed that it was a very common proceeding to apply a large lead plaster (spread upon leather) to the breast in cases where it becomes necessary to get rid of the milk. This, of course, rendered friction and all meddling impossible. He had found that two large and soft handkerchiefs suitably applied, one by way of a sling around the neck, under the breast, the other in exactly the reverse way over the breast and tied round the body, so as to include the breast between them, interposing a large pad of cotton wool, to constitute a very efficient mode of applying pressure.

Dr. Edis remarked that it was important, in cases of threatened mammary abscess, to resort to every expedient sanctioned by experience, and not to rely wholly upon any single one. In cases where the child was stillborn or died shortly after birth his usual method was to apply a belladonna plaster 18 inches by 6 across the mammary region—a hole the size of a penny piece being cut to leave the nipples free—the day following parturition, before the breasts had time to fill, abstinence from fluids and great moderation in diet being required for the first few days, an aperient mixture composed of Mag. sulph. with the Pot. Iod. being given twice or thrice daily to relieve the bowels. The shoulders should be raised, the arms kept perfectly quiet, the upper part of the chest being only lightly covered; any friction or "drawing the breasts" being strictly prohibited. Where this method had been adopted he had never seen a single instance of mammary abscess. An evaporating lotion continuously applied to the mammæ was in some instances sufficient to prevent the secretion of milk; but the pressure obtained by the plaster was of great service and effectually prevented the employment of any friction.

ANNUAL MEETING.

At 9 o'clock the Scrutineers retired, and on their return the President declared the following gentlemen elected as Officers and Council for the ensuing year :

Honorary President.—Sir Charles Locock, Bart, M.D.

President.—William Overend Priestley, M.D.

Vice-Presidents.—William Frederick Cleveland, M.D. ; Edward Copeman, M.D. (Norwich) ; Thomas F. Grimsdale (Liverpool) ; Alfred Meadows, M.D. ; W. S. Playfair, M.D. ; Charles H. F. Routh, M.D.

Treasurer.—Gustavus C. P. Murray, M.D.

Honorary Secretaries.—Alfred Wiltshire, M.D. ; Arthur W. Edis, M.D.

Honorary Librarian.—James H. Aveling, M.D.

Honorary Members of Council.—Henry Oldham, M.D. ; Robert Barnes, M.D. ; John Hall Davis, M.D. ; Graily Hewitt, M.D. ; John Braxton Hicks, M.D., F.R.S. ; Edward John Tilt, M.D.

Other Members of Council.—George Granville Bantock, M.D. ; John Bassett, M.D. (Birmingham) ; William Bloxam, M.D. ; George B. Brodie, M.D. ; Thomas Chambers, M.R.C.P. Ed. ; William Henry Day, M.D. ; James Ellison, M.D. (Windsor) ; John H. Galton, M.D. ; William Chapman Grigg, M.D. ; William Newman, M.D. (Stamford) ; John Baptiste Potter, M.D. ; George Roper, M.D. Henry Cooper Rose, M.D. ; Heywood Smith, M.D. ; Arthur B. Steele, L.K.Q.C.P.I. (Liverpool) ; Thomas Taylor, F.R.C.S. (Birmingham) ; John Williams, M.D. ; J. Lucas Worship (Sevenoaks).

The following report of the Auditors for the year 1874 was then read, and on the motion of Dr. Cumberbatch, seconded by Mr. Dunn, unanimously adopted :

BALANCE SHEET OF THE OBSTETRICAL SOCIETY OF LONDON.

Dr. *Abstract of the Receipts and Expenditure for the year ending December 31st, 1874.* Cr.

1874.		£	s.	d.
January.	To Balance in hand from 1873 account	139	5	5
	(1). Subscriptions received during 1874:—545 at £1 1s.	572	5	0
	(2). Composition Fee.....	10	10	0
	(3). Midwifery Examination Fees .	4	4	0
	(4). Sale of 'Transactions'	76	8	0
	(5). Interest on 3 per cent. Consols £1284 8s. 9d.	41	0	1
		£843	12	6

				£	s.	d.
By (1) 'TRANSACTIONS':—						
Printing, Lithography, Paper, and Binding, making Index, Delivery of 'Transactions,' &c.....				236	5	3
(2) LIBRARY AND MUSEUM:—						
Library—Purchase of Books and Binding	77	8	7			
Furniture (Table and Bookcases)	8	0	0			
Rent, Salaries, and Petty Expenditure.....	237	10	6			
				322	19	1
(3) GENERAL EXPENSES:—						
Meeting-room, Expenses of Meetings, Stationery, Collection of Subscriptions, Postages, &c.				122	18	0
Balance in hand carried to next year's account				161	10	2
				£843	12	6

Audited and found corrrect,

WHARTON PETER HOOD,
WILLIAM HOPE, } *Auditors.*

January 5th, 1875.

The following report of the Honorary Librarian for the year 1874 was then read, and unanimously adopted on the motion of Dr. Langmore, seconded by Dr. Heywood Smith.

Report of the Honorary Librarian for 1874.

Mr. President and Gentlemen,—The yearly record which it is my duty now to lay before you contains nothing eventful; nevertheless the information I have to give is exactly what every Fellow would wish to hear, for all the items of it tend to prove how increasingly useful the Library and Museum of this Society are becoming. The proceedings of the past year may with satisfaction be compared with those of any prior year. They prove that a larger sum than usual has been spent on books; that more works have been presented; that more visitors have entered the Library; and that more books have been taken out of it than in any previous year.

Further accommodation for books has been demanded, and a new book-case has in consequence been added. A table for the microscope has also been provided, and a cabinet for specimens will probably be required.

A portrait of Peter Chamberlen has been presented by Dr. Taylor, of Birmingham; and an exceedingly interesting cast of an antique group representing the circumstantials of labour by Dr. S. H. Bibby.

The ordinary work of the Library continues to be carried on efficiently and smoothly; and the Fellows may be congratulated in possessing such a magnificent collection of books relating to the branch of medicine in which they are particularly interested.

J. H. Aveling, M.D.

A CASE OF LABOUR COMPLICATED BY PELVIC TUMOUR AND BY CONVULSIONS.

By Henry M. Madge, M.D.

This case is another illustration of the difficulties attending labour complicated by tumour in the pelvis. They are cases which must always cause the greatest anxiety to the attendant. Even the first examination reveals the unpleasant fact that there is an obstruction to labour which must be attended with difficulty and danger —a sort of case that will haunt the unfortunate possessor of it night and day until it is over. I have already brought two somewhat similar cases before the Society, the details of which are recorded in our 'Transactions.' In the first (vol. iv, p. 129) the tumour occupied nearly the whole of the pelvis. It was opened and pushed above the brim, but the patient died in a few days from peritonitis, which appeared to have for its starting-point the seat of the puncture. In the second (vol. xiv, p. 227) the uterus was studded with fibroids, which however, only partially impeded labour. In this case I was able to deliver the patient, with forceps, of a living child, and she made a good recovery. In the case I am about to relate the patient had, as another serious complication, puerperal convulsions, which, of course, necessitated immediate action.

Mrs. L—, æt. 24, primipara, short and stout, rather dark complexion; always enjoyed fairly good health, but has suffered a good deal of pain occasionally at the catamenial periods. The earlier months of her pregnancy were attended with an excessive amount of sickness. This almost entirely disappeared after the fifth month and she then went on nearly to the full term, to all outward appearances, in the most promising manner. About a fortnight before the expected time she slipped her foot and

fell forwards on getting out of a cab; slight labour pains, with gradual escape of liq. amnii, came on the next day (June 3rd). On the following day the pains continued, but still weak and irregular. On making an examination I found the posterior wall of the vagina pushed forward by a tumour, about the size, as far as I could judge from that portion of it within reach, of a large orange. It seeemed to have a smaller mass attached to it. Although it was hard it was somewhat yielding to the touch, but without fluctuation. Examined per rectum the tumour was found lying in Douglas' space and felt much the same as per vaginam and perfectly immovable. Labour had made but little progress, the head was resting on the brim, os uteri dilated to the size of a florin, and the parts moistened with escaped liq. amnii. Seeing that the case might be protracted and attended with difficulty I was glad to get the assistance of my neighbour Dr. Wells, of Fitzroy Street. As the patient was well and in tolerably good spirits, the pains feeble, and the external parts rigid, we thought it advisable to retard rather than to hasten labour. A dose of laudanum was given which secured a good night.

June 5th.—The pains reappeared and seemed to be getting stronger and more frequent, but up to late at night the head had not descended; the os uteri, however, was more dilated. During the day we made several ineffectual attempts to push the tumour above the brim. At length we decided to leave it alone, hoping that when the stronger expulsive pains came it would yield sufficiently to allow the child to pass. Turning was thought of, but rejected on account of the escape of the liq. amnii. We also discussed puncturing the tumour; but with the recollection of my former case we did not think favorably of it. Tr. Opii was again given at night, but the sleep was disturbed by occasional pains.

June 6th.—Matters remained much the same, the labour went slowly on during the day, and at night the os uteri was more dilated and the head somewhat lower.

The patient kept up wonderfully well, took nourishment freely, and as yet showed no signs of flagging strength or spirits.

June 7th.—Took no opium last night, and has had but little sleep; pains getting quicker and stronger, and it seemed now pretty evident that the real struggle was about to begin. In three or four hours the head was much lower in the pelvis, the os uteri pretty fully dilated, and the tumour apparently undergoing compression and flattening, when unfortunately the patient had a violent attack of convulsions, followed in a short time by a still more violent attack. Turning being now still more out of the question on account of the lower position of the head, chloroform having been administered by Mr. Bailey, Dr. Wells applied the long forceps. This was easily accomplished, but neither his efforts nor mine, pulling with all our might, could make the head advance. We then had recourse to craniotomy, and after a long difficult manipulation of upwards of an hour the patient was delivered of a well-developed full-sized male child. It was found on removing the placenta, which came away easily, that during the passage of the child's head and body, the tumour had been forced up above the brim and could not anywhere be felt. If it had been of ovarian origin it would probably have been felt through the abdominal walls; my impression is that it was a fibroid attached to the posterior surface of the uterus by a pedicle sufficiently long to enable it to drop into the pelvic cavity. The chloroform acted admirably in arresting the convulsions and keeping the patient quiet. For a few days, partly, perhaps, from the chloroform and partly from the convulsions, the patient was confused and semi-delirious, but this all passed away and in a fortnight she was up and doing well. The day after the delivery I had occasion to use the catheter. The urine had no albumen, so that the convulsions were probably due to a passing congestion of the brain, brought about probably by a strikingly short, thick neck. Some years ago I attended a labour accompanied

with convulsions in the Talbot Road, Baywater, where the conditions were much the same—short neck, non-albuminous urine, and where strong labour pains had only lasted about an hour. Of cases of puerperal convulsions associated with albuminuria I have had several in the course of my practice. In two very bad cases I had the valuable aid of the late Dr. Tanner, and all the patients recovered. I may mention in passing that, in both the cases where Dr. Tanner was called in, he ordered lemon-juice to be given freely. In the case I have now related of course there are many points as to diagnosis and treatment or management open to discussion, but whatever opinions may be advanced I am glad to know that my patient was assisted safely through two of the most serious complications that the obstetrician has to grapple with.

Dr. Murray inquired if the tumour had been removed subsequently.

Dr. Cumberbatch asked what was the supposed therapeutic value of lemon-juice in convulsions.

The President then delivered the Annual Address.

ANNUAL ADDRESS.

Gentlemen,—It is a time-honoured custom to begin an Annual Address by paying a passing tribute of respect to those who have been removed from the midst of us during the previous year. We have to deplore the death of fourteen Fellows; most of them threw themselves so earnestly into the engrossing cares of medical practice, that they had no time to make themselves known to fame, and this will account for my having little to say of them.

Edward Bousfield, of Thetford, in Norfolk, studied at Queen's College, Birmingham; he became a L.R.C.P. Edin. in 1866, and joined us in 1868. He settled at Thetford, was made Justice of the Peace, and died, much regretted, at the early age of 35.

Newton Lee, of Hornsey, studied at St. Bartholomew's, became a M.R.C.S. in 1857, and joined us in 1859. He died last January, aged 52.

Mathias David, of Pendre, Cardigan, was a L.R.C.P. Lond., and M.R.C.S. He joined us in 1871, and died last June, aged 52.

George Mendenhall, M.D., of Cincinnati, America, who joined us in 1872, and died last year, was a man of considerable note, he gave an address before the American Medical Association at Washington in 1870, and I regret my inability to tell you more about him.

Edmund Snell, of Stepney, became M.R.C.S. in 1845, L.R.C.P. Edin. in 1859, and he joined us in 1869. He was in practice, at Mile End, for twenty-seven years, and he died, aged 53, regretted by a host of friends.

Alfred James, of Forest Hill, became M.R.C.S. in 1859, M.D. St. Andrew's in 1862, and he joined us in 1868. He was medical officer of the Forest Hill Dispensary, and he died last August, aged 45.

Frederick Abercromby Hope Robson, of Iver, Bucks,

studied at University College, became F.R.C.S. in 1869, M.D. of Brussels in 1867, and he joined us in 1868. Dr. Robson was a superior man; his connexion with the P. and O. Company induced him to settle at Shanghai, and he gained great credit for the pains he took in the defence of the town against the rebels. When his health gave way in China, he took a country practice at Iver; but a constitution once damaged by a tropical climate cannot cope with the weather changes that must be faced in country practice, and he died at the early age of 39, while actively engaged in all sorts of useful work.

Robert Ransom, of Cambridge, studied at University College, became F.R.C.S. in 1858, M.D. St. Andrew's, in 1862, and he joined us in 1860. Dr. Ransom was a man of great local influence, one of those highly commendable men who, by becoming members of our town corporations, are able to persuade them to adopt sanitary improvements, which would have been otherwise indefinitely deferred. The best thing, however, to be said of him, is that hundreds of the poor lined the road to see their faithful friend passing to his last abode.

Herbert Robey Harris, of Bury, became M.R.C.S. in 1859, and joined us in 1861. He had previously served in Australia as medical government officer at King George's Sound. He died last January of typhus, caught in discharge of duty, aged 42.

An officer knows that, if he falls in battle, the country will give a pension to his wife and children. A medical man knows that, if he falls fighting against infectious diseases, his country will leave his wife and children penniless. Such is the difference of the reward meted out to men, according as they elect to wear a red or a black coat.

Henry Digby Delamotte, of Swanage, was born at Holywell in Oxfordshire, in 1796; he studied at the Middlesex and at the Windmill Street schools of medicine. He took no degree, but joined us in 1861. He was long admiralty surgeon at Swanage, and a poor law

medical officer for fifty years. Having had to tell you of the premature demise of so many men, it is perfectly refreshing to me to be able to say that Mr. Delamotte had no reason to complain of his constitution. As for walking-stick and glasses, the comforts of old age, he never felt their want, and at the age of 77 he died suddenly, without ever having been ill, just as he was going upstairs to see a patient. His loss was felt as a public calamity, and it was said of him "that no form was more familiar in the houses of the poor or more welcome in the houses of the rich"—a most enviable epitaph for a medical man!

Charles Christopher Hayman, of Eastbourne, became M.R.C.S. in 1850, M.D. Aberdeen in 1853, and he joined us in 1859. He was in good practice in Kent in 1854, when hæmoptysis caused the apex of the left lung to be found diseased. He, therefore, removed to Eastbourne, which re-established his health, a benefit he repaid by the active part he took in the sanitary improvements of the place. Forgetting that, when once seriously damaged, a man is not fit for great work, he overtasked his strength, and Bright's disease came on some years ago, when he retired from practice, and he died last August, aged 48.

I now come to one whose pleasant face was often seen at our meetings. Thomas Ballard, of London, was educated at St. George's Hospital, became M.R.C.S. in 1843, M.D. of St. Andrew's in 1862, and he joined us in 1859. He has written on syphilis, and on the value of bleeding in apoplexy, and several works to show that "sucking when an infant can thereby get little or no food is a great source of disease to both child and mother." This singular notion did not prevent Dr. Ballard being a good practitioner; and he was so much respected that you placed him on your Council, that he was Vice-President of St. Andrew's Medical Graduates' Association, and President of the Harveian Society. The consoling lesson to learn from this is, that, provided a man's pathology be otherwise fair, and his professional conduct perfectly straight-

forward, he may ride a hobby with considerable vigour without losing the esteem and the respect of his fellow practitioners.

Frederick Bird, of London, was born in Colchester, in 1818, became F.R.C.P. Edin. in 1841, and M.D. St. Andrew's in the same year. He was made M.R.C.P. in 1859, and joined us in that year. He was for many years obstetrical physician and lecturer on diseases of women, to the Royal Free Hospital, and afterwards to the Westminster Hospital. When he died a marked presentment disappeared from the West-end streets; for you must remember the high raised coachman, towering above a silver-plated chariot, and inside, the middle-aged gentleman with refined features, dressed as for a party, always alone and sitting well in front with his hat off. What a contrast to the style and manner of his elder brother, Dr. Golding Bird, with his clerical dress and parsonic cast of features, and nevertheless both were eminent and successful men. Beneath a display so unusual in the 19th century, there was a man, who besides the courage the fop has often shown on the field of battle, had a keen intellect, asserting itself chiefly in the precision and in the rapidity of his diagnosis of pelvic diseases, besides other qualities that make a very successful practitioner. Like most of the men who succeed in London practice, when F. Bird settled in town, in 1842, his energies were sharpened by the stings of poverty, and the following year, when only twenty-five years of age, he was bold enough, without having ever witnessed the operation, to perform his first case of ovariotomy, and he remained the chief London operator till the opening of St. Mary's Hospital in 1852.

His operations were openly performed in presence of Blundell, Rigby, Benjamin Phillips, Sir Charles Locock, and many men of less note; nevertheless the report spread that he concealed his unsuccessful cases. This originated in his refusal to give Dr. Robert Lee* the details of his

* "An Analysis of 108 Cases of Ovariotomy," 'Med.-Chir. Transactions,' vol. xxxiv.

thirteen cases of ovariotomy, when, in 1850, that gentleman was preparing a paper on ovariotomy, for the Royal Medical and Chirurgical Society. It would have been doubtless more courteous to have done so, but surely a man may reserve the details of his cases for his own use without incurring the charge of unfair dealing. Another reason which led to F. Bird's being thought to deal with his ovariotomy cases in an underhand manner was that, already in 1850, he had come to the three following conclusions:—1st, that to decide whether it was judicious to perform ovariotomy it was sometimes necessary to make a small exploratory incision; 2nd, that this incision was comparatively innocuous; and 3rd, that these cases of exploratory incision should be excluded from ovariotomy statistics. These conclusions, which F. Bird had arrived at twenty-five years ago, are substantially admitted now; but when, in 1850, in the discussion* that followed the reading of Dr. Lee's paper, F. Bird came out with the statement, that besides the thirteen cases of ovariotomy to which he owned, he had often made exploratory incisions, the senior surgeons were loud in protesting that all incisions into the peritoneum should be counted as cases of ovariotomy, and that not to do so was to suppress cases. In the 'Lancet'† of the following week F. Bird published an outline of all his cases up to that time, and in accordance with the ruling of his critics, in the discussion alluded to, he included eighteen cases in which he had only made an exploratory incision, and he subsequently published some valuable papers‡ on the "Diagnosis, Pathology, and Treatment of Ovarian Tumours." This discussion and the concluding remark of Mr. Lawrence, "that ovariotomy might soon be looked upon as an attack on the character of the profession," threw a damper on ovariotomists, and F. Bird no longer laid himself out for ovariotomy, although he occasionally operated, and for the last

* 'Lancet,' 1850, vol. ii, p. 583.

† Ib. ib., p. 592.

‡ 'Medical Times and Gazette,' 1843.

time in November, 1871. He lived to witness the surgical audacity of Baker Brown, at St. Mary's Hospital, again draw attention to ovariotomy, and to see its recognition as a legitimate operation fully established at home and abroad by the cautious and scientific practice of Mr. Spencer Wells and Dr. Thomas Keith. I say abroad, for the Continent refused to accept the verdict of American surgery till it had been ratified by British surgeons. I maintain that F. Bird's conduct with regard to ovariotomy was perfectly honorable, and that he no more concealed his fatal cases than his successful ones; but I regret that he shirked the trouble of recording his latter cases. Whether this was the result of overwork, idleness, or disgust at unfair treatment, he must still be blamed, for even when a man feels himself most unjustly accused, he has no right to wrap himself in contemptuous silence. The course he upholds or the science he cultivates is for a time identified with him, and he is bound to publish all he knows on contested points, trusting that the future will rectify contemporary injustice.

It seems that neither Frederick nor Golding Bird were strongly enough constituted for the mental and physical wear and tear of London work, for they both died comparatively young. In 1873, F. Bird had a succession of ill-defined ailments that were set down to gout; he became more and more prostrate last year, although he rested now and then. He died last year of broncho-pneumonia, aged fifty-six.

It is with mingled feelings of pleasure and regret that I now bring to your recollection the memory of one, by all esteemed, and beloved by many of us. John Jones Phillips was a Welshman by family and birth; he was educated at Mill Hill School, matriculated in 1860, and was articled to Mr. Pye-Smith, who has had the great honour to see four of his pupils become physicians to Guy's Hospital.

Dr. Phillips passed an unusually brilliant examination for the licence of the Royal College of Physicians, became

a member in 1868, and took the M.D. degree at the University of London. He was a gold medallist in obstetrics, and it was to this branch of medicine that his studies and his practice were chiefly directed, although he had spared no pains to become well informed in the whole range of medicine. He was appointed demonstrator of anatomy to the school of Guy's, settled in practice in Finsbury Square, and in 1869 he was made assistant obstetric physician to Guy's. In the same year he succeeded Dr. Barnes as physician to the Eastern Division of the Royal Maternity Charity, the largest obstetric institution in England. Shortly afterwards he became consulting obstetric physician to the Tower Hamlets Dispensary, and assistant-physician to the Hospital for Sick Children in Great Ormond Street. In all these onerous offices he worked with only too much zeal. He filled the office of Secretary to the Hunterian Society and of the Obstetrical Society, and to the latter post he had been re-elected for a third year only a few days before his death. He contributed several valuable memoirs to our 'Transactions' and to the Transactions of Guy's Hospital. Amongst these may be mentioned the following:—"On the Natural History of Ovarian Disease, with Post-mortem Records of eighty-eight cases," 1867; "On Retroflexion of the Uterus as a Cause of Abortion," 1872; "On the Mortality after Obstetric Operations," 1871; "On Sudden Death from Syncope after Labour," 1873; "On the Treatment of Puerperal Convulsions without Bleeding," 1873. He could with difficulty be prevented from writing a work on diseases of women, and consoled himself by adding to his labours the joint editorship of the 'Guy's Hospital Reports.'

While doing all this work he knew that ever since his student days he had an organic disease of the heart. His friends also knew it, and plied him with good advice, which was always courteously received, but with most gentle obstinacy he pursued the arduous course he had chalked out for himself. In the early summer of 1873 he

received a more impressive warning in the shape of an attack of aphasia, and to the surprise of those who were not in the secret, he went to Scotland for a few weeks, returning in October with scarcely impaired speech. Still increasing rather than diminishing his engagements, he had been for some time sleeping out of town, placing his nights' rest at the mercy of an hysterical patient, when, on his return home, in the morning of the 28th of January, he complained of headache and sickness, and, though he saw some patients in his house, was not able to go out. Symptoms of cerebral disturbance, which were significant enough, probably appeared less important to him, from their having passed off so readily a few months before; and, forbidding his servant to call in any of the friends who were close at hand, and would have been eager to give what help could have been possible, he went early to bed, saying that he should be better after a night's rest.

When called next morning he was unconscious, and the colleagues, who were soon around his bed, found him in deep coma, with contracted pupils, stertorous breathing, and paralysed limbs. The old apex murmur was still audible, and there can be little doubt that embolism, or a fragile state of the arteries, had led to extensive cerebral hæmorrhage, filling the ventricles and pressing on the medulla. He never regained consciousness, and died about three in the afternoon of the same day.

Such is an outline of the brief career of our lamented friend. Instead of vainly picturing to ourselves what he certainly would have become as a consulting man, let him be pourtrayed in our annals just as we have so often seen him in this room—the very model of a secretary. With great aptitude for business, he was never in a hurry, but always had his work well in hand; he was as judicious in council as in debate and a charming reader. With a due sense of the ridiculous, he knew how to stifle in its birth the wounding smile, so as to be ever courteous to all; and his features habitually bore that impress of earnest-

ness and of unaffected kindness that are so prepossessing in the handsome and the young.

Having thus concluded the most important part of my task, I have only a few remarks to make on the present state of the Society.

To make up for our losses by death, resignation, and erasure, we have last year recruited 48 new fellows, and, including our 27 honorary fellows, we now muster 657 strong. I am fairly entitled to congratulate the Society on its last year's work. The interest attending our discussions has been above the average, particularly when the presentation of numerous specimens led to short and lively discussions, in which many took an impromptu part. I hope the remembrance of these spirited meetings will impress on all fellows the importance of forwarding specimens to our secretaries, and, by so doing, our absent country fellows will take a most effective part in the work of the Society. The new volume of 'Transactions' will reflect the good work of last year; it will also contain an index of our fifteen volumes, for which we have to thank Dr. Potter, and I am sure it is unnecessary for me to enlarge on the nature of the benefit he has thus conferred on the Society. With regard to our finances, your treasurer is adding some £50 to your invested capital, although we have been liberally spending upon our library, which should be gradually made complete in all that relates to obstetrical and gynæcological literature.

Independently of our work as a scientific society, you know that we have been occasionally obliged by our metropolitan position to take the lead in questions relating to the welfare of obstetric medicine, and last year we were called upon to decide whether our laws permitted us to admit women to the fellowship of the Society. Your almost unanimous decision that women were not admissible was one of great importance, for the profession felt that although the question was tried on a technical point, your verdict really meant that women were not qualified by nature to make good midwifery practitioners; that

they were unfit to bear the physical fatigues and the mental anxieties of obstetrical practice at menstrual periods, and during pregnancy and puerperality, and that it was unfair to society to encourage women to suppose that they could ever fit themselves to assume responsibility in those formidable obstetric emergencies which too often completely paralyse men of experience as midwifery practitioners.

The remembrance of your decision on this question may also remind you that last year I explained to you that the lower classes of society were at the mercy of uneducated midwives, uncontrolled by aught else than the fear of a coroner's inquest. Last year Lord Aberdare and Mr. Stansfeld were in office; they favorably entertained the views repeatedly brought forward by the Society, and they were prepared to bring in a bill for the better education and registration of midwives, when a change of ministry convinced your Council that the question must be left in abeyance.

It was after mature deliberation that the Obstetrical Society of London determined to move in this matter; its original resolve has been sanctioned by successive councils, and it will be for the new council to decide whether it is not time to reopen the question with the present ministers.

Ours is the only civilised country that has left unregulated the midwifery of the humbler classes, and it is a disgrace to obstetric medicine that there should be no means of preventing an incompetent and drunken woman from assuming the name and the duties of a midwife. There is every probability that our efforts in this direction would be favorably received by a Government that has no great measures to carry, and which seems bent on dealing earnestly with all questions relating to public health.

Gentlemen, I cannot vacate the chair without thanking you for the invariable courtesy you have shown me in the discharge of my duty; and I likewise thank the various

officers with whom you have associated me for having rendered easy and pleasant the duties of office. I am glad my place will be taken by one so well qualified as Dr. Priestley to preside over your meetings, to further the interests and support the honour of the Obstetrical Society of London.

It was then moved by Dr. Braxton Hicks, and seconded by Dr. Routh—

"That the best thanks of the Society be presented to the retiring President, Dr. E. J. Tilt, for the able and efficient manner in which he has presided over the meetings of the Society during his term of office, and that he be requested to allow his address to be printed in the 'Transactions.'"

This was carried by acclamation, and the President thanked the Society.

Dr. Langmore moved, and Dr. Aveling seconded, a cordial vote of thanks to the retiring officers, which was carried unanimously.

FEBRUARY 3RD, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—54 Fellows and 8 visitors.

Books were presented by Dr. Alfonso Bos, Dr. G. Calderini, Professor A. Casella, Mons. Leblond, Drs. Hegar and Kaltenbach, Dr. G. L. Ponza, and Dr. Ely van de Warker.

Dr. William T. Greene and Dr. F. J. Hannan were admitted Fellows of the Society.

The following gentlemen also were declared admitted: Mr. E. Barber (Sheffield), Dr. J. Barclay (Banff), Dr. W. Blair (Jedburgh), Dr. A. Fraser (Aberdeen), Dr. E. L. Koch (Ceylon), Mr. W. Owen (Ebbw Vale), and Dr. W. A. Satchell (Kew).

Professor Gustav Simon, M.D. Heidelberg, and Professor Courty, M.D. Montpellier, were elected Honorary Fellows of the Society.

The following gentlemen were elected Fellows: A. Cordes, M.D. (Geneva), Richard John Morton, M.R.C.S. (Aylsham), Ernest Watson Paul, L.K.Q.C.P.I. (Brixton), and Richard Thomas Smith, M.D. (Haverstock Hill).

The following gentlemen were proposed for election: Dr. R. J. M. Coffin, Mr. C. J. Cullingworth, Mr. J. H.

Ewart, Mr. P. B. Giles, Dr. A. Jukes, Dr. A. E. A. Lawrence, Dr. E. Rey, Mr. F. W. Salzmann, Dr. C. Liebman, Dr. D. E. Seton, and Mr. F. W. Strange.

PRESIDENT'S ADDRESS.

In taking the Chair, which by your kind favour I occupy for the first time this evening, I am anxious to express my sincere thanks to the Society for according to me a position so distinguished.

When my nomination to the presidency of the Obstetrical Society was first mooted, I confess I felt many misgivings as to my competence for such a responsibility. It is true that some twenty years' experience as an obstetric teacher and practitioner may give me a claim to be numbered among the seniors of the profession, but the pressure of other occupations and considerations of health, have for a considerable period obliged me to abstain from attending the meetings of this and other societies, and consequently I am less familiar with the modes of procedure, than some of our more eminent fellows who have "borne the burden and heat of the day," and who might justly look forward to the President's Chair as the reward of excellent service to the Society.

My objections on these points were overruled, and when I understood that it was the earnest wish of the Council that I should be put in nomination, I could no longer hesitate to submit myself to the wish of the Society, and to accept what was so spontaneously offered. It remains for me to tender my acknowledgment of the honour conferred upon me, and to thank both the Council who nominated, and the Fellows of the Society who have ratified its selection. Whatever of diligence and of zeal may be required of your President, I shall do my utmost to bring to the discharge of the duties of this Chair, and

aided by our experienced secretaries I may hope to conduce to a satisfactory dispatch of business, and a general advancement of the objects contemplated in the foundation of the Society.

Having been one of the original members, one of the small band who in 1858 met to consider the propriety of instituting an obstetrical society in London, I have watched with much interest its development from infancy to maturity, and the success it has achieved may well be a source of pride to its Fellows. There is, I believe, no parallel instance in the history of a medical society in this country of such rapid growth, in so short a period, as that which has taken place in the Obstetrical Society. In the short space of sixteen years it has enrolled well nigh 700 Fellows, and the annual recurrence of the 'Transactions' testifies to the large number of active workers and contributors. The fact indeed has become apparent that the Society supplied a want which had previously been indefinitely felt by a large number of medical practitioners, and which only assumed the concrete when the Society was founded.

It has been said that every practitioner regards himself as an expert in obstetric practice, which perhaps is only another way of saying that most general practitioners have felt the tug and strain on their resources in midwifery practice, the necessity for immediate action in moments of peril, and that most have some victories to chronicle, some recollection of difficulties surmounted, which have left more vivid impressions than any other kind of practice. Be this as it may, the fact that by far the largest proportion of medical men in this country are engaged more or less in the practice of obstetrics, and in treating the diseases of women and children, affords a broad basis for the interest which is felt in a society whose object is to promote improvements in the obsetric art, and its influence permeates and pervades the mass of our medical brethren in proportion to the importance of the subject to them, and its bearings on their daily work.

Reviewing the history of the Society, young as it may be, I cannot doubt that it has done much good, and achieved something by way of advancing the science and art of obstetric medicine. The number and diversity of the contributions contained in the 'Transactions,' the long record of pathological specimens, the exhaustive discussions on special topics, are all evidences of the zeal possessed both by town and country Fellows, and an appreciation of the scientific value of their contributions is shown by the sale of the 'Transactions,' and by the frequency with which they are translated or transcribed into the literature of other countries.

But I may be permitted to point out that in proportion as the influence of the Society has grown as an exponent of gynæcological knowledge and opinion at home and abroad, so is its responsibility, and the greater the care which should be exercised on the part of the Fellows, in the examination of opinions which if promulgated, may possibly, though perchance unintentionally, form the groundwork for mischievous errors in practice.

I have often been struck with the curious current perversion of the views of some author who may have taken especial pains to make himself understood, and to guard against misconception. In the University of London examination for degrees in medicine, nothing was more common than to attribute to the late Sir James Simpson the practice of separating the placenta in all cases of *placenta prævia*, when it is well known to those who have studied his papers that he only recommended this remedy in some unusual and embarrassing conditions. But if errors may be promulgated from mere misunderstanding, how much more mischief may arise from teachings which are themselves erroneous or pernicious. Enunciated under the auspices of this Society, and promulgated in the provinces wherever the proceedings are read, the results may be disastrous both to doctor and patient, and hence it behoves each Fellow to guard both himself and the Society against the acceptance of error which otherwise might

become as mischievous as widespread. Caution is more especially necessary in reference to operations which involve important issues either to the life or future health of patients, and I venture with some diffidence to indicate one or two quicksands or dangers in the present state of gynæcological science.

It is unfortunately in the very nature of things that exaggerated or partial views should be entertained when a new idea or a fresh method of treatment is developed, and mere enthusiasm often sways earnest men from one extreme to another. By way of illustration I may point out that when midwifery forceps became so perfected that in good hands they could be used in most cases with safety both to mother and child, and it became established that, as a rule, the use of instruments need not be deferred so long as heretofore, some zealous practitioners began to throw aside all precautions, to trust as little as possible to nature, and, by way of saving time, to have recourse to instrumental delivery more frequently than desirable. In like manner one ingenious practitioner, struck by the way in which difficulty in the delivery of some head presentations was overcome by version, actually proposed, if the passages were sufficiently dilated, to turn in all cases of head presentation whenever the least delay occurred in the progress of natural labour.

In the department of women's diseases there has from time to time been a tendency to give prominence to some single pathological condition, in such way as to shut out, or at least obscure, other and perhaps equally important affections of the uterus and its appendages. We have had an epoch in which the ovary was assumed to be the prime factor in all uterine ailments; we have had a period when inflammations and ulcerations of the os and cervix uteri held the foremost place in uterine pathology; and now we are threatened with a reign of uterine displacements, in which the majority of symptoms of womb derangement are attributed to flexions and versions of the organ, and, influenced no doubt by current medical opinion, patients

as they present themselves in the consulting room appear stricken with an epidemic of displacement, and imagine that all their discomforts are caused by dislocation of the womb,—just as at one time the sufferers from uterine symptoms generally believed themselves to have ulceration; neither supposition perhaps being in accordance with the facts. The promulgation of particular views in pathology, even if erroneous, would lead to little harm if it did not culminate in hurtful methods of treatment, or tend to lower our prestige in the eyes of the public, and in those of our brethren who practise in other departments of our profession. But when theories carried into practice involve proceedings which may possibly be dangerous in their immediate or remote results, or which necessitate frequent vaginal examinations, great circumspection is required in their adoption.

Imagine what would be the result if the celebrated professor who first practised division of the cervix for dysmenorrhœa and sterility, were to find many imitators without his genius, who began extensively to incise the cervix in most cases of painful menstruation, or where pregnancy was desired! Or picture what would result if it were generally believed that nearly all forms of uterine disorder could only be cured by the frequent repetition of some form of cauterization; or, again, if a large body of medical practitioners were to become possessed of the single idea that uterine displacement was the root of evil in the majority of female ailments, and that pessaries of some kind were essential to cure both married and single women so suffering!

I would beg especially not to be understood as depreciating the labours of some of those eminent pioneers who have done so much to advance our knowledge of uterine disease. It would be in the last degree unjust not to acknowledge how much we are indebted to their conscientious labours, and there is great temptation to give preponderance to subjects which have occupied so much attention, and have cost much pains in investigation. But

I am anxious to guard against the acceptance of some single idea or theory which, if ridden as a hobby, may hinder the advance of a broader and more comprehensive uterine pathology, and perchance lead to meddlesome and mischievous methods of treatment. The mind, preoccupied with a sole idea, is apt to search only within the limits of that idea, and to overlook or ignore what may be more important, but beyond and outside it.

In an experimental art like ours practice goes through phases and fashions in accordance with prevailing principles in pathology and therapeutics, and we are not alone in this respect. The practice of physic has had its phases of bleeding, blistering, and antiphlogistics, followed by a diametrically opposite stimulating method. A mercurial plan of treatment has had its day, and has been succeeded by an anti-mercurial plan, and so forth.

Scientific men are sometimes charged with being so conservative as to be unwilling to entertain new suggestions which are out of the beaten path. This seems to me a groundless charge, so far as our own profession is concerned. Its members show the greatest aptitude to receive fresh accretions to their resources, if what is new affords reasonable evidence of genuineness. The danger is perhaps in the opposite direction. The recent and rapid progress in the physical sciences has raised expectation too high; and has sometimes favoured a too ready acceptance of novelties, which later experience does not approve.

Without undervaluing recent improvements, it has seemed to me, in making a general survey of our ground, and weighing our present position, that the great impetus given of late years by many admirable workers to the progress of uterine surgery has tended to throw the balance somewhat too much over to the surgical side of the scale, and that operative and mechanical methods of treatment have displaced somewhat unduly and hurtfully the medical and psychical considerations in uterine cases.

It is essential to the proper exercise of our art that,

while we lend an attentive ear to every suggestion of improvement, in action we should be conservative in the truest sense of the term, particularly when interference may possibly do more harm than good. I need not recall to the initiated the dangers which beset operations in midwifery, and the reasons which induce the wise practitioner to abstain from interference except when the necessity is imperative. The current axioms about "Meddlesome Midwifery" are as true in our times as in former days. Experience also has abundantly proved that even minor operations on the unimpregnated uterus cannot be undertaken with the same immunity as like operations in external surgery. Simple incision; the removal of a polypus; an intra-uterine injection; the insertion of an intra-uterine pessary, have not infrequently been followed by grave symptoms, which in some cases have terminated in the death of the patient, or in the permanent maiming of the genital apparatus by peritonitis.

It is no uncommon experience for a young gynæcologist to start full of ardour for operative procedures, which he believes will cut short the slower and more cautious methods of cure, but ere long he discovers that even slight operations cannot be performed without hazard, and if he has a conscientious regard for the welfare of his patients he grows more conservative as he grows older. It is true we have witnessed great achievements in gynæcological surgery, brought about by indomitable courage and infinite pains in the face of great perils. I know no more striking illustration of this than the success which our distinguished *confrère*, Mr. Spencer Wells, and, following him, Dr. Thomas Keith, have attained in ovariotomy. But it must be remembered that this operation has been undertaken and perfected to avert the progress of a disease which ere long inevitably terminates in death. The risk incurred bears a direct relation to the magnitude of the peril in which the patient stands before the operation, and there is no comparison admissible with hazardous operations which are undertaken for the alleviation of some minor ailment,

or possibly for the cure of a woman who is barren, but otherwise in perfect health. While, therefore, the sagacious and prudent gynæcologist should shrink from nothing which will promote the ultimate good of his patient, he should not readily be beguiled into what is new or adventurous without sufficient proof of its necessity, efficacy, and reasonable security, and without well considering whether some lesser measure will not bring about the same result. Further, he should be conservative of his resources in diagnosis as well as in treatment. He who as a rule employs the uterine sound without considering whether the whole circumstances are such as to afford *primâ facie* evidence that it is necessary to clear up some doubt, will certainly inflict a large amount of pain and annoyance on his patients, with a minimum of good result. The same observation applies in some degree to the use of the speculum, and without being bound by any slavish rule on the subject, I may go so far as to say that any method of treatment for uterine affections, always supposing it to be efficient, the more readily commends itself, if it does not entail too frequent examinations per vaginam.

This Society, consisting as it does of so many who are eminent as obstetric teachers and practitioners, may with propriety act as moderator when new views are propounded on matters within its province. While it gives countenance and encouragement to all sound innovations which combine improvement with comparative safety, it may assess, so to speak, their true value and importance, and its judicial decision will command respect, in proportion to the care and caution of its deliberations.

From this aspect I regard the position of President as one of high responsibility, and I look forward with some diffidence to the exercise of the trust you have confided to me. I must, however, rely on your forbearance, and hope that by showing a strict impartiality I may win your approval, and at the same time guide the debates to a wise decision.

If the Society has already done much, there is yet ample work in store for it. Besides accumulating the valuable records of experience, there are numerous interesting subjects of inquiry which might make the reputation of Fellows who have aptitude for them. There are yet extensive mines of investigation which have been but very partially worked, and which would well repay time expended on them. Take for example the diseases of the placenta; notwithstanding what Simpson, Barnes, and others have done, much remains yet to be learnt. The whole subject of "intra-uterine death," comprising the pathology, causes, and prevention of abortion, and of the death of the fœtus in the later periods of pregnancy, is one full of interest, and offers a field for investigation of great extent.

Take again the subject of septicæmia or blood-poisoning; how little we know as yet beyond what is conjectural. How little we know of the nature or of the origin of the poison itself, and yet how serious we know its results to be, and how helpless the wisest of us to cope with it when extensively developed. Surely something might be done by careful experiment and chemical analysis combined, to elucidate the subject, and aid its prevention or mitigate its effects.

One topic of great practical interest on which further information is required has been confided to a committee of able observers nominated by the Society to investigate and report upon.

I allude to the subject of *Transfusion.*

The report is, I understand, nearly ready, and when it is presented it may be desirable to set apart an evening for its discussion, so that the opinions and experience of Fellows may be elicited on the general question of Transfusion and its value in actual practice.

One of your presidents has called attention to the comparatively small number of contributions to the Society on infantile pathology. I agree in thinking this is to be regretted. The diseases of young children are commonly

regarded within the province of the obstetric physician, and their treatment forms so large and important a section of work, in the practice of every family doctor, that their careful and constant study becomes desirable on every available opportunity. It is well known that, from certain structural and physiological peculiarities, children are subject to diseases which differ in many important respects from those of adults. Their pathological anatomy differs remarkably from that of adult patients. If attacked by the same disease as the adult, the common ailment will in accordance with the tender age of the patient be modified in its course, be attended by other symptoms and complications, and perhaps terminate in a different manner. Special modes of diagnosis have to be employed in investigating children's ailments, and special therapeutics employed in their treatment.

In view of the importance of the subject, I trust that Fellows may be persuaded to bring more contributions before the Society concerning infantile pathology, and that the records may be found in next year's 'Transactions.'

Professor CASELLA, of Milan, presented his transfusion apparatus to the Society.

Dr. T. C. HAYES exhibited a uterus and appendages which were connected by adhesions, in several places, to the large intestine. The patient had not been pregnant for some years, and had suffered greatly from menorrhagia. She had been under the care of Dr. Johnson in King's College Hospital, and the uterine history was somewhat incomplete.

M. C—, æt. 42, married twenty-one years. She has had two living and healthy children, and four miscarriages. Not been pregnant for some years, whereas she has been subject to frequent and copious menorrhagia, which has

been much agravated during the past year or so. Lately she has had three or four attacks of hæmatemesis, and her breath has been distressingly short.

She lived in India several years, where she had ague and rheumatic fever.

When admitted into hospital she was suffering from cardiac disease, contraction of liver, and enlargement of spleen, associated with ascites and œdema of legs. Death occurred after an attack of hæmatemesis.

There was no reason to suppose that she had been intemperate, nor was there any clear evidence of her having had syphilis.

Autopsy.—Large serous effusion into peritoneum. The liver much contracted, weight thirty-seven ounces. Its surface was nodulated, and in parts, where the capsule was thickened, broadly and deeply fissured. Under the microscope, it was seen that the cells were fatty and that the interstitial fibrous tissue was considerably increased.

The spleen was hypertrophied and congested, and its capsule, in several places, was opaque and thickened; weight twenty-six ounces. Stenosis of mitral valve, without any fibrinous deposit.

Lungs emphysematous.

The uterus was drawn upwards and to left side, in consequence of its attachment to colon. Per vaginam, it was felt to be dragged up, but it was fairly moveable.

The uterine cavity was of normal length, but the muscular tissue was firmer and thicker than usual. The thickness of wall of body was one inch, and that of the neck half an inch.

The mucous membrane of the uterus was quite normal, except near the opening of right Fallopian tube, where for the extent of about a threepenny piece it was extremely congested, and had lying on it a small semifluid clot. There was a broad and firm band of adhesion, about two and a half inches in length, uniting the right corner of the fundus to the caput cæcum coli, and four to five small fibrinous chords, about one and three quarters inches in length, and

precisely like the chordæ tendinæ of the heart, ran from the appendix vermiformis to the broad ligament, just behind the origin of the round ligament from uterus. The sigmoid flexure of the colon was closely adherent, for about two inches, to the left border of the fundus, just behind the round ligament, and as high up as the point of emergence of Fallopian tube from uterus. The right half of the posterior surface of the uterus, from the Fallopian tube to the os internum, was connected by a loose cellular tissue to the anterior surface and sides of rectum. The left half of this surface was quite free.

The left ovary was seen behind and at the side of the uterus; its anterior surface was firmly attached to the adhesion, uniting the sigmoid flexure of the colon to uterus, and from it a small fibrinous band passed to the rectum. The ovary equalled in size a large walnut, and contained two cysts, filled with fibrinous clots and dark sanguineous fluid. These bulged forward to the posterior surface, whereas the anterior surface had a natural appearance.

The right ovary was hidden from view, being imbedded in the adhesion uniting the uterus and rectum, and on separating the fibrous envelope, the ovary was about same size as the other, and also cystic. Both Fallopian tubes were perfectly occluded, bristles could only be passed from uterine openings into left one five eighths of an inch, and into right two and a half inches. The fimbriated ends of the tubes were not visible.

Dr. Hayes observed that it would have been almost impossible to have made out the condition of the uterus during life, for on vaginal examination the uterus was remarkably moveable. No doubt the menorrhagia was passive, and arose from the impeded portal circulation, and probably was intensified by the serious obstruction to the return of blood from the uterus caused by the adhesions. Hence the necessity of looking beyond the uterus, at times, if we wished successfully to explain and cure symptoms. The cause of the adhesions was quite obscure, and by

their obliteration of the Fallopian tubes they must have effectually prevented impregnation.

Dr. PRIESTLEY inquired whether there was any history of previous pelvic peritonitis. The amount of pelvic adhesions, without any history of this occurrence, was a point of interest.

Dr. HAYES replied in the negative.

Dr. EDIS alluded to a case of his recently published in the 'Lancet,' where morbus Brightii was the exciting cause of profuse menorrhagia; this latter condition being often dependent upon the most varied conditions, both general and local.

Dr. EDIS exhibited for Dr. Diver an apparatus called the couchaid, designed to assist labour and economise force during parturition, enabling the patient, by pulling during her pains, to give comfortable pressure to the back, and support to the uterus at the same time. It consisted of a cushioned back pad, and another for the front, with two stirrups for the feet, and a series of connecting straps and cords.

Dr. AVELING thought the ingenious apparatus for accelerating labour just exhibited ought to be used with the greatest caution. He feared that, more particularly in the earlier stage of labour, if not carefully applied, it might have exactly the opposite effect to the one desired; and as the patient herself had to use it, it was necessary that she should be at the time composed, or at least quite mistress of her actions. A severe pain might either cause her to abandon it altogether or use it too violently. He thought pressure by the hand preferable.

Dr. MURRAY objected to the apparatus on the score that it would be advertised as recommended by the Obstetrical Society. He thought we had sufficient to attend to in looking after the mother and child without complicating matters by applying any such apparatus.

Dr. EDIS exhibited an ovum expelled about the eighth week, showing the villi of the chorion. The decidual membrane had not been passed.

The PRESIDENT remarked it was a pity the outer coats had not been obtained to make the specimen complete.

Dr. Edis showed an india-rubber speculum-trough he had designed to fit over any speculum so as to prevent wetting the bed in cases where it was necessary to employ the syringe in gynæcological cases. He had tried it in several instances, and found it answered the purpose well. It was similar to the ordinary ear-trough, except that, being elastic, it could readily be attached to any speculum, and did not interfere with any manipulations that might be necessary.

REPORT OF THREE CASES OF CEPHALOTRIPSY (WITH TWO CASTS).

By J. Braxton Hicks, M.D., F.R.S., &c.,

OBSTETRIC PHYSICIAN AND LECTURER ON OBSTETRICS AT GUY'S HOSPITAL; EXAMINER IN MIDWIFERY AT THE ROYAL COLLEGE OF PHYSICIANS, LONDON.

With the reports of three cases of cephalotripsy I venture again to present to this Society two more casts of fœtal heads (delivered by the cephalotribe) taken before the removal of the instrument in order to enable its action to be seen in the varying positions of the head.

It will be readily seen on examining the casts that I employ the instrument as a tractor as well as a crusher. The incurving of the ends, as designed by Sir James Simpson, answers this end excellenlty, though perhaps in his instrument the incurved portion was too long. If the total width of the blades be measured in these casts, the outside measurement is far less than the smallest diameter of any pelvis where delivery by the natural passages is proper to be attempted. If, as has been advocated in France, the blades were removed after the crush had been accomplished, the head would have re-expanded and the effect of the crush been partly lost; besides which another instrument would be required as a tractor, and thus the

manipulation would be much increased. Still, I would not lay it down as an absolute rule that we are to deliver by the cephalotribe, because in certain cases circumstances may make it advisable to remove it, and draw down by other means, or even by turning. Such a case is the third one here given; the reasons are set forth in the report. However, in nearly every case we can employ the cephalotribe in the twofold manner with decided advantage.

Of course, the casts show the maximum size of the compressed head; the minimum must be judged by observing the part not completely crushed and calculating the amount of compressibility natural to it.

The cephalotribe has not yet become so popular as it deserves to be; and this, I am sure, is from the want of the knowledge of its value. A collection of casts will be the best means to point out at a glance its power, and reports of cases its use. I therefore add my quota. I feel confident that it will, in the main, supersede crotchet and craniotomy forceps in all cases of severity. I may add that the instrument I use is that already shown to this Society, of the type of Sir James Simpson's, but much stronger and with different screw arrangements.

The first case is represented by the cast, and occurred in a stout, unmarried, and apparently wide-hipped primipara, aged about thirty. However, when labour had existed for some hours it became clear to her attendant that she could not deliver herself. He sought the assistance of a friend, who, having tried the long forceps without success, perforated the head. Both crotchet and craniotomy forceps were tried, but the head could not be made to descend. After two hours I was asked to assist, and found not only the brim contracted, but that the bony outlet was so small that the hand could not be passed up to the brim. The tuberosities approximated to two and a half inches; however, behind them there was room for the cephalotribe to pass, and thus I managed to seize the head in the manner shown in the cast, and,

after a quarter-turn, by steady traction drew it down, the larger or facial portion coming down posteriorly. The advantage of the instrument as a tractor in this case was incalculable, for the other tractors could not succeed; indeed, from the contraction of the outlet they could not be fairly brought to work.

This case terminated unfortunately. The feverish symptoms, which had begun before interference was employed, did not subside after; nor did she clearly become conscious after the withdrawal of the chloroform, under which she had been for some hours. Gradually she became more feverish; pain was complained of in right side. At the end of the fourth day she was unconscious, and died in a comatose state on the fifth day after delivery.

The second case was that of an Irishwoman, married to a Japanese. She was about thirty-five years old, and had been delivered with extreme difficulty in her two former labours by craniotomy. She was at full term in this her third labour; the child presented by the breech, causing a very tedious labour. After thirty hours the body was delivered, but when the head engaged the brim detention ensued, and though as much traction was used as was deemed safe it could not be drawn through. I was asked to see her. I found the occiput to left side in front, and the conjugate contracted to about two inches. I perforated the head, behind the ear, with some difficulty, owing to its hardness, and then tried to draw it through. I exerted as much force as I considered safe, yet without advance; I then employed the cephalotribe. The body of the child was pulled well on to the perinæum, and the blades passed up either side of the head in the transverse direction. I thus seized it in a line from the occiput to the forehead over the ear, as shown in the cast, and having applied the screw, the head was crushed without any difficulty, the brains escaping through the opening at the base; I then gave a quarter-turn and drew down. For a moment the larger portion, as shown in the

cast, although brought into the left side-loops, produced a little resistance, but this was overcome in a few seconds, and the head was delivered in about three or four minutes after first use of the instrument. As the head descended the thicker part referred to, having come through the side-loop, descended posteriorly over the sacrum. It is in the cast about three inches wide, and would represent the antero-posterior diameter of the loop, if we allow a slight reduction for compressibility, say to about $2\frac{1}{2}$ to $2\frac{3}{4}$ inches. The space between the blades, together with the blades themselves, is $1\frac{5}{8}$ inch.

The use of the crusher-tractor is well exemplified here, because, if the cast be noticed, it will be seen the parts of the head not under the line of crushing are indrawn towards that line, and thus the total diameter of these uncrushed parts is diminished. If the blades are removed, then the included portion expands, and also at the same time the adjacent parts. As before noticed, if we take off the cephalotribe we must employ another tractor, which certainly increases the irritation and risk; where the head is after-coming there is, of course, a possibility of doing without crotchet, yet there is no advantage in any way in employing that mode, and possibly much disadvantage.

In this case no spiculæ of bone obtruded themselves. The woman was quite well in a fortnight.

The third case occurred in a primipara, about 22 years old, and about 4 feet 3 inches high; she was small-sized in every way, and hollow backed. She had been many hours in labour under a midwife, who sent for Dr. Ashburton Thompson. He at once detected the difficulty of the case, and sent for his partner, Dr. Brunton, to assist, who, seeing it impossible to deliver by forceps or turning, perforated the head, and tried to draw it down by the crotchet and craniotomy forceps, without success; the bones broke away without advance. After trying two hours with various forms of tractors they asked me to assist with the cephalotribe. I found the conjugate diameter about two inches, perhaps one eighth more;

the transverse, although comparatively much better, yet below the standard; the cavity and outlet much below. It was, however, not difficult to reach the brim. The vaginal portion of the cervix was elongated, descending into the vagina half-way down, increasing much the difficulties of the case, for the scalp hung down in a similar manner, rendering it very difficult to discriminate one from the other, being also pressed closely together.

I first removed some portions of broken bone, and applied the cephalotribe in the transverse diameter. This was done twice; but as the resistance was but slight, I concluded that I had only attacked the vault. I again applied it, and this time with full effect; but on examination I was afraid to draw down, because of the outstanding fragments of bone, in so small a space. I therefore removed the blades, and set myself carefully to remove every portion liable to injure. After this was over I passed the crotchet outside the skull and caught the orbit. I drew down the face without great difficulty, and the head soon escaped. But our difficulties were by no means over; the smallness of the pelvis in all directions caused much difficulty to the descent of the chest; however, by the crotchet one shoulder was drawn down, and the arm drawn out with great opposition, and then the other also with nearly as much. However, it was overcome, and the rest of the labour was concluded without trouble.

This patient made an excellent recovery, and was, a fortnight after, about in her room in good spirits.

The sacral promontory was the principal cause of the obstruction, though the lumbar vertebræ assisted much in throwing the head forward.

The *external* measurements I found to be as follows:

Extreme transverse ilial . .	11 inches.
Between the anterior spinous processes	10 „
Antero-posterior . . .	6 „

The *internal* measurements of the conjugate diameter I found to be $2\frac{1}{8}$ inches at the outside,

Now, it has been considered by many authors that we should take off three inches from the external measurement of the antero-posterior diameter in order to arrive at the conjugate. This may be sufficient for the normal pelvis, but, so far as my experience leads me to judge, it is not sufficient for distorted pelves, especially those of the reniform kind. In this case we ought by that reckoning to have had a conjugate of three inches, whereas it was nearly one inch less. In another case I measured we had for the sacrum alone four inches; the external measurements with the three-inch deduction gave an inch more than actually existed, as proved by direct measurement.

Dr. Murray inquired if Dr. Hicks had ever known the instruments to slip. It seemed to him (Dr. Murray) that its principal value was that of a tractor; as to pressure on either side, it was merely a change in the bulk of the head. He had never used the cephalotribe himself: but if the head were not perforated, lessening it in one way would only increase it in the other. Without wishing to disparage the instrument he thought that if he were able to attain sufficient power with the hook and forceps, delivery would be quite as easy as with the cephalotribe, especially as Dr. Hicks said it would supersede the craniotomy forceps or blunt hook after perforation.

Dr. Playfair said that Dr. Murray's reasons for preferring the old craniotomy forceps and crotchet to the cephalotribe must be that he had never, as he admitted, used the latter instrument. Any one who had done so must at once admit its immense superiority; and it was strange that even after many years of use teachers recommended the crotchet and craniotomy lever for ordinary cases, instead of teaching, as they ought, that the cephalotribe was the proper resource whenever it could be used. No better proof could be given of its value than the test of Dr. Hicks' case. Here was a case in which two experienced operators and medical officers of one of our largest obstetric charities had failed to deliver with the older instruments, when the cephalotribe succeeded. Could any evidence of its value be stronger? One particular value of the apparatus was also well illustrated by his paper, namely, the safety to the maternal soft parts, which were effectually preserved from injury by the spiculæ of bone, in consequence of these being within the scalp; hence the danger arising from picking away the broken bones was entirely avoided.

Dr. Heywood Smith said that one of the chief advantages of

cephalotripsy was the rapidity with which delivery was accomplished, and this was an element of great importance, for the cases were generally those where labour had already been prolonged until the mother's powers were becoming exhausted, and where every minute saved was of the utmost importance. He, unlike Dr. Murray, had never performed craniotomy proper, and in all the cases where he had been compelled to perform cephalotripsy the facility of delivery was one of the remarkable features.

Dr. Roper.—After perforation?

Dr. H. Smith.—Of course.

The President remarked that he had often been struck in operating with the uselessness of many instruments. Where the skull was firm even Sir James Simpson's cranioclast was useless. With the cephalotribe the bones were broken down more readily, and a firmer hold was obtained than by other means. In some cases the spinal hook of Dr. Oldham was most efficient.

Dr. J. Braxton Hicks, in answer to Dr. Murray, said that the head must always be perforated before cephalotripsy. If this was not possible it was not a case for delivery *per vias naturales*. He had found the head expand after removing the instrument, not because of the brain within, because by passing the perforator three or four times through it its resistance was overcome, but it expanded from its own elasticity tending to recover the globular form. This he had proved by frequent experiment, and therefore he had many times in this Society pointed out the advantage of drawing down by the cephalotribe, in opposition to the French recommendation that it should be removed and allowed to be expelled by natural efforts or drawn down by crotchet. With regard to the remarks which fell from the President, he said till he had employed the cephalotribe he was much indebted to the cranioclast of Sir James Simpson. But with regard to the use of Dr. Oldham's vertebral hook, and the fixing the crotchet in the foramen magnum, he thought there had been much misapprehension. With regard to the crotchet, it could not pass into the vertebral opening; but if it could catch the sella turcica, or if Dr. Oldham's hook were passed into the foramen magnum, this did not assist the descent of the head, for if say the three inches diameter of cranial base had to pass through say two inches of pelvic brim, if we pulled on the centre of the base, the position was not altered; and as three inches of base after removal could not possibly pass through two inches, any amount of pulling would not conquer the difficulty. All that was required was the bringing down the head side- or faceways. From some unaccountable error a constant answer from students to the question, "How would you draw down the head after perforation?" was, "By the vertebral hook." No other instrument was thought of. The vertebral hook was invented by Dr. Oldham to seize the head

which had been torn off the body in breech presentations by being passed into the spiral foramen of the remaining vertebræ. With regard to the advantage of the cephalotribe over crotchet and craniotomy forceps, it must be remembered that these instruments only can act by virtue of the compression of the mutilated head against the vaginal walls, and therefore the pressure on the walls of the passage was in direct ratio to the traction employed, whereas the cephalotribe by compressing the head directly removed all pressure and friction in a fourth of the time, and even in some cases in a tenth, it would take to deliver by the old plan.

Dr. Hayes stated that some French authors advocated clearing out the brain before applying the cephalotribe. Dr. Hicks seemed always to stir the brain up well and so break it up.

HÆMORRHAGE INTO THE PELVIS.

Dr. Heywood Smith exhibited the uterus and ovaries of a woman who had died the previous day, within fifteen minutes, of hæmorrhage into the pelvis. She was 26 years of age, single, had been confined sixteen days previously. On the third day had a sharp attack of puerperal septicæmia, from which she with difficulty recovered. She had been sitting up for two days, when suddenly, about 12.30 p.m. on Tuesday, February 2nd, she became faint, struggled violently, became blind, and died in less than a quarter of an hour, retaining her consciousness until the last. At the post-mortem examination, which was only very cursorily made, the pelvis was found full of blood. The specimen showed the uterus insufficiently contracted, probably the result of the attack of septicæmia, the left oviduct adhering to the broad ligament as the result of recent inflammation, and on the left ovary a small opening. The right ovary was studded with small cysts. Both ovaries were larger than natural. The specimen had not yet been carefully examined, but it did not seem very evident as to whence had issued the fatal hæmorrhage.

Dr. Bloxam remarked that, given the conditions stated by Dr. Heywood Smith, those who were familiar with the symptoms of pelvic hæmatocele, and with the fearful organic shock attending the outpouring of blood into the peritoneal cavity, would expect such a patient rapidly to succumb.

The President requested Drs. Playfair, Bloxam, and H. Smith, to examine the specimen further with the view of discovering the source of hæmorrhage.

NOTE ON THE TREATMENT OF CHLOROSIS AND ANÆMIA WITH THE PHOSPHIDE OF ZINC.

By J. Ashburton Thompson,

FELLOW, SURGEON-ACCOUCHEUR TO THE ROYAL MATERNITY CHARITY.

A young lady, æt. 18, returned from school in Germany presenting the appearance of chlorosis. The menses had failed to appear at the proper terms for five months. She complained of lassitude, excessive drowsiness, loss of appetite, headache, occasional sickness, and constipation of the bowels, and of pains in the back and legs excited by the least exertion. The pulse was feeble, soft, and slow; occasionally she suffered from palpitation, and the *bruit de diable* was audible over the veins of the neck. Sometimes she had a trifling amount of neuralgic pain in one or other side of the head. These symptoms had come on during the last three months, and she had been under treatment (with some ferruginous tonics) during the greater part of that time; yet, although she got no worse, she had made no improvement. She then came under my care. She was directed to take an acid preparation of iron three times daily, and with each dose half a grain of zinc phosphide. At the third day of treatment she noted decided improvement. At the eighth day she desired to discontinue treatment—she said she was quite well. Her appearance was unaltered, although there was perhaps a little more colour in her cheeks; but her disorder was still

obvious to the eye. The bruit was undiminished, the skin still yellowish-white and pasty, and the tongue large, pale, and indented ; but, on the other hand, her appetite was now normal, the drowsiness had departed, energy returned, and pain no longer ensued upon the exertion which she now felt fitted and even anxious to undertake. In short, the subjective symptoms were entirely removed in this short space of time, although the objective symptoms remained unmodified. At the end of another fortnight, or on the twenty-third day, the patient refused to continue the treatment. Three months from this date she returned, complaining of the same symptoms as before. The menses had appeared about six weeks since, rather profusely. The same treatment was again adopted ; and in the course of a week she recovered from her feelings of debility so completely that, from being entirely disinclined to take walking exercise, and, as she averred, actually unable to walk a mile without suffering severely for it, she was able on the sixth day of treatment to dance all night. It is well to note here that there were no symptoms of hysteria.

I have briefly recapitulated the points in this example of chlorosis because, although they differ in no way from those of other cases of the kind, I look upon it as a typical illustration of the peculiar powers of phosphorus in a certain class of cases. I need scarcely say that the very rapid improvement in the subjective symptoms here described is such as is not usually, if ever, observed under ordinary treatment. As does the removal of the objective symptoms, so, as a rule, does the removal of the subjective symptoms, depend upon a long-continued course of appropriate tonics carefully administered, nourishing diet, and regular exercise and occupation. But the difficulty which thus-afflicted persons experience in taking food or exercise, or, indeed, in engaging in any occupation, is no doubt one of the chief obstacles to their recovery, as well as one of the most difficult points of treatment ; and therefore, although the drug here recommended may not

be actually remedial of the condition on which these symptoms depend, it is not the less valuable as an adjuvant, removing the disabilities of these patients in a remarkably rapid manner. But I do not assert that it will be found to act in every case with the marked rapidity with which it acted in this one; I relate this as a typical example.

I said that this instance illustrated the powers of phosphorus in a certain class of cases. I referred to all kinds of anæmia, whether the result of hæmorrhage or of deficiency or malformation of blood. Accordingly I beg leave to draw the attention of this Society to the value of phosphorus in the treatment of patients recovering from uterine hæmorrhage. In these cases, as in the foregoing, nothing is more remarkable than the facility with which results similar to those above described may be attained. In a few days of treatment at most, the blanched patient, who, if the stomach have not actually rejected food, at least has an invincible loathing for it, *feels* comparatively well; I do not mean to say that she feels strong again, but that she loses her former sensations of discomfort, and the appetite is speedily restored to a very considerable extent. Thus recovery is facilitated. Neuralgia is a common accompaniment of this condition, and it is over this neuralgia that phosphorus exerts that power which I have been tempted to call specific, so speedily and unerringly is it manifested. But I do not know whether the term can justly be applied to this action, for the neuralgia is of that sort which is generally amenable to an alcoholic stimulant. Phosphorus in a certain dose is a stimulant too; and the fact that it is most effective when given in such a dose points, perhaps, rather to a general than a specific action of it. The result, however, is too permanent—for the attack, once thus dispelled, rarely returns—to be exactly comparable to that procured with alcohol; and the conclusion that it operates in some other manner too cannot be avoided. Possibly this mode of action may consist in a

power of modifying the state of the capillary circulation, in support of which view some arguments might be adduced. But the ground which is common to both these kinds of anæmia—the hæmorrhagic and the constitutional —is the simple ground of deficiency in the blood of the metalloid in question. This is not only a constituent of healthy blood, but has been specially ascertained to be deficient in the blood of anæmic persons. So, as iron is administered in such cases as those under consideration with the result of increasing the proportion of iron in the blood, phosphorus may be administered for a similar purpose. But, further, as the result of the administration of iron is not only that, but an actual increase in the number of red blood-corpuscles—that is to say, a stimulation of the processes of nutrition—so phosphorus may be given to this end also. Phosphorus, however, has this advantage over iron thus used, that, in the first place, experience shows that it is much better adapted to this ultimate end of encouraging the general nutrition of the body, while, in the second, it serves the immediate end of obviating symptoms which are not only distressing in themselves but serve to retard the prime, essential, process of cure. These appear to be the results attained in such cases, and I have now come to regard phosphorus as indispensable to their proper treatment. Finally, I may be permitted to remark, although it can scarcely now be necessary, that free phosphorus is not the treacherous poison it has hitherto been considered. It is a fatal and potent poison, it is true, but its therapeutic effects may be obtained with precision and perfect safety. If proper formulæ be employed, no apprehension of unexpected or uncontrollable poisonous effects of a therapeutic dose need hinder its general employment.

Dr. Routh said it might be known to some members of the Society that he had made several inquiries on the employment of phosphorus. In some cases it produced marvels, specially in cases, like those detailed by Dr. Thompson, due to deficiency of phosphorus in the system. But it occasionally acted injuriously,

producing headache and giddiness, and in a few cases (of idiosyncrasy perhaps) it acted as a deadly poison even in the first dose, sometimes immediately producing vomiting and syncope. The safest preparation was the phosphide of zinc, but it was a medicine always to be closely watched when given.

Dr. TILT inquired what preparation of phosphorus Mr. Thompson recommended.

Mr. THOMPSON, in reply, said Dr. Routh had revived the old objection, not because it was a poison, but because we cannot calculate the results. These arose from decomposed phosphorus.

Dr. PLAYFAIR inquired whether the zinc phosphide was not insoluble in pill.

Mr. THOMPSON replied that some acid tonic given simultaneously would serve to assist the decomposition.

THE TREATMENT OF RIGID PERINÆUM, AND THE AVOIDANCE OF ITS RUPTURE.

By H. ERNEST TRESTRAIL, F.R.C.S., Aldershot.

WE meet with ample instructions in many of our standard works for the dilatation of the os uteri, but, so far as I am aware, the same line of treatment has not been applied to the perinæum, to which, however, I find it peculiarly applicable; this probably arises from the long-established idea that the perinæum needs *support*.

In many cases I have found the following plan succeed where otherwise the forceps would have been required; in fact, whenever I now meet with a long, firm perinæum, I materially shorten the labour by hitching two or three fingers into the posterior commissure, and keeping up extension; by this means not only do I succeed in speedily enlarging the outlet, but I prevent any rupture of the perinæum, an accident far more frequent than is usually suspected.

The following case is an example of the benefits to be derived from this treatment:

I was called to Mrs. L—, a primipara, at 4 a.m., and

found that she had been in labour fourteen hours. The pains had been very strong and regular up to 3 a.m., but had since then been gradually decreasing in force. The patient was much exhausted. The os uteri was fully dilated, the vertex resting upon the perinæum, which was long and unyielding. Here was a case urgently calling for assistance. The uterus tired out. The perinæum rigid. The patient exhausted. To have administered antimony or chloroform would have been madness. To have given ergot, or to have applied "vis à tergo," quite unwarrantable. To have resorted to small incisions, utterly useless. To have used the forceps would probably have resulted in a ruptured perinæum.

In my way of thinking the only justifiable course was, first to remove the obstacle, and then to arouse and assist the uterus. With this view I hitched three fingers into the posterior commissure, and kept up pretty firm, *continuous* extension, first with one hand and then with the other. In less than half an hour the outlet was very considerably dilated. I then gave ten minims of the Extractum Ergotæ Liquidum, applied a bandage firmly around the abdomen, and assisted each pain by pressing with the hand over the uterus. In a very short time I had the pleasure of seeing a fine boy, and of knowing that my patient was in no way lacerated.

I am an advocate for the early application of the forceps, in suitable cases, but am convinced that by *first* dilating the perinæum in the way indicated, *even when the obstruction is at the brim*, we may not only save time, but avoid those ugly rents for which we are always (shall I say justly) more or less blamed.

CASES OF PROLAPSUS.

By Dr. A. Cordes.

Case 1.—Mrs. S—, a poor widow, æt. 56, came to me on the 2nd of October, 1873, requesting me to take away an old pessary which she had worn two years on account of a prolapsus of the womb.

With much trouble, and a pretty good amount of strength (being necessary), I pulled out the most abominably stinking "pessaire au gimblette" I ever met with. In doing so I got a small wound in the right index finger, which I had to have cauterized with chloride of antimony.

On the 21st of December, 1873, Mrs. S— came again to me, complaining that "the womb falls down again" when she walks. On examination I found the womb coming down whenever the patient strains, and causing her much discomfort.

Knowing that Dr. Vulliet had contrived a pessary for prolapsus, I requested him to place one of them. He kindly did it on the same day.

(The part *b* of this instrument was placed behind the cervix, in the posterior *cul-de-sac* of the vagina, the part *a c* in the anterior *cul-de-sac*, the cervix being so between the *appendix* and the ring.)

Since then the patient went well. Almost every day I see her lifting burdens, carrying heavy bags of chips and fagots, from which she gains her livelihood.

Case 2.—Mrs. M—, a widow, æt. 54, came to me on the 2nd of September, 1874, asking relief for her sufferings; she states that, since she was twenty years of age, "the womb is outside." She complains of frequent micturition; no actual pain.

On examination I found a large tumour between the

thighs, protruding about two inches, covered by the inverted vagina, which had a cutaneous aspect, being dry and bright. The anterior wall of the vagina is quite inverted, and with it the bladder, forming a pouch in which I felt urine fluctuating. (See Barnes's 'Clinical History of the Diseases of Women,' Fig. 105). No vaginal portion. The whole of the tumour is as large as the normal uterus, not very sensitive, and is easily replaced. No adhesions.

Os uteri looking backwards, red, and the seat of a trifling oozing. (Sound enters six centimètres.) The uterus being reduced, and the bladder emptied, I placed a Vulliet's pessary. The one I first placed being a little too large, I replaced it by another, a little smaller. When I took away the first I ordered the patient to cough, to strain, to stand up, without the uterus falling out. However, I placed a small Vulliet's pessary. Since then, I saw the patient several times; everything is in order.

January 11th, 1875.—Patient came again; no suffering, no inconvenience, except frequent micturition owing to displacement of the pessary, which, being too small, lies transversely across the pelvis and does not keep the uterus high enough. I placed another one a little larger.

SIDE VIEW OF THE PESSARY.

a, anterior part; *b*, posterior part; *a–b*, ring; *a–c*, appendix; *a–d*, *a–c*, ascending branches; *c*, posterior incurvation of the appendix; in *c* is a small groove for the urethra. The appendix supports the anterior vaginal wall.

MARCH 3RD, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—40 Fellows and 6 visitors.

Books were presented by Dr. George Johnston and Dr. L. D. Bulkley, and a fine copy of Bidloo's large work on 'Human Anatomy,' with plates, published in 1685, was presented by Mr. Michael Blood, of Jersey.

Mr. Alex. H. Brewer and Dr. Richard Thomas Smith were admitted Fellows of the Society, and the following gentlemen were declared admitted :—Dr. Aug. Cordes, (Geneva) ; Mr. Ernest Watson Paul (Titchfield) ; Dr. Harry Wells (Gualeguaychee) ; and Dr. David Young (Florence).

The following gentlemen were elected Fellows of the Society :—R. J. Maitland Coffin, M.R.C.P. Ed. (Malta) ; Charles J. Cullingworth, M.R.C.S. (Manchester) ; John H. Ewart, L.R.C.P. (Manchester) ; P. Broome Giles, junr., L.R.C.P. Ed. (Hereford) ; Augustus Jukes, M.B. (St. Catherines, Ontario) ; Alfred E. Aust Lawrence, M.D. (Bristol) ; Charles Liebman, M.D. (Trieste) ; Eugenio Rey, M.D. (Turin) ; Frederick W. Salzmann, M.R.C.S. (Brighton) ; David E. Seton, M.D. (South Kensington) ; and Frederick William Strange (Aurora, Ontario).

Edwin Hollings, L.R.C.P. Ed., was proposed for election as a Fellow.

Report of sub-committee on an ascitic fœtus exhibited by Mr. Ashburton Thompson.

The ascitic fœtus referred to us for examination was in a state of decomposition. The epidermis was loose and could be easily peeled off. The tissues generally were œdematous, the walls of the abdomen were especially so. The head was separate from the body. The peritoneal cavity contained a few ounces of serous fluid, in which were seen floating some shreds of fibrin. There did not appear to be any obstruction to the circulation of the blood through the heart.* The liver and kidneys were soft and pulpy, the peritoneum was œdematous, so that it was not possible to guess at their condition during life. Under these circumstances we were not able to discover the cause of the ascites.

John Williams.
J. Ashburton Thompson.

CLINICAL NOTES ON THE EARLY COURSE OF CANCER OF THE CERVIX UTERI.

By Charles Liebman, M.D.,
PHYSICIAN (MEDICO-CHIRURGO PRIMARIO) TO THE CIVIL HOSPITAL, TRIESTE.

Communicated by Dr. Tilt.

All authors, even those who have made the most extended researches in the pathology of uterine cancer, confess to knowing nothing about its beginning, which seems generally to be gradual and slow; and either unac-

* The orifices of the hepatic veins in the inferior vena cava were ascertained to be patent.

companied by suffering or presenting symptoms so little marked as not to attract the patient's notice.

The question being still unsettled, which of the physiological textures—the epithelium (Waldeyer), the cells of the connective tissue (Virchow), or the endothelium of the lymphatic vessels (Köster)—is the original seat of the disease, and the instances of incipient carcinoma of the cervix seldom occurring to clinical observation, our deficient knowledge of the earlier stages of this growth is easily explained.

We cannot rely on the descriptions of primary cancer uteri given by ancient writers, who may have confounded the most different growths and alterations of tissue with carcinoma; but, from the first accurate notices of Teallier to the most recent treatises, all agree in opinion upon the point that the neck of the womb is the part of the organ which is most frequently attacked by cancer. If we, however, examine the opinions of the various pathologists as to which part of the cervix is considered to be generally the first to undergo cancerous degeneration, we find that almost all believe the vaginal portion, its lips, or the edges of the os externum, to be the part which shows the first symptoms of the disease.

Almost everything that has been written on incipient cancer of the neck is referable to the degeneration of its lower end; and, in the opinion of all clinical authors, the greatest diagnostical difficulties exist especially in cases of non-ulcerated cancer of the vaginal portion, inasmuch as it may be mistaken for benignant hypertrophy (metritis colli chronica). To quote only some eminent writers, Scanzoni,* Churchill,† Gusserow,‡ Gallard,§ consider the differential diagnosis between these two morbid states nearly impossible, while Spiegelberg,‖ lays great stress on

* 'Die Chronische Metritis.' Wien, 1863. P. 183.

† 'On the Diseases of Women.' Dublin, 1867. P. 386.

‡ 'Sammlung Klin. Vorträge,' No. 18. Leipzig, 1871.

§ 'Leçons Cliniques sur les Maladies des Femmes. Paris, 1873, p. 598.

‖ 'Archiv für Gynäkologie,' vol. iii, p. 233 and following.

two points; these are, the non-dilatability of the cancerous os by sponge tents, and the immobility (nicht verschiebbarkeit) of the mucous membrane, in cases of cancer.

It is, however, a fact that the cancer may begin in the cervical canal itself, and this perhaps is more frequent than generally believed. The fact is not unknown, but, in my opinion, it has not yet met with the attention it deserves; its recognition appears to me very important for the diagnosis of the disease, and in some cases it must influence our therapeutical proceedings.

The works of some of our principal clinical teachers, as far as they are at my disposal, afford the following notices on the subject:

Kiwisch * relates a few cases in which cancer began near the os internum (von der Gegend des inneren Muttermundes ausging), and spread downwards; in these instances the os externum was the part of the neck which was last invaded by the growth.

At a post-mortem examination of an old woman Virchow† found a cancerous degeneration of the whole cervical canal; the disease had reached the os internum, whilst the edges of the os externum were healthy. Nothing was known of the history, but Virchow remarks that, if an examination by the speculum had been made during life, it would not have been possible to diagnosticate the cancer.

Wagner,‡ in his work on cancer of the womb, quotes a few cases in which the disease seems to have originated in the mucous membrane of the cervical canal; but in one case he describes an old alcoholic preparation at the museum of Leipzig, in the other he quotes short extracts from previous authors that are not clear and accurate enough to convince us.

* 'Klinische Vorträge,' &c. Prag, 1854. Vol. i, p. 510.

† 'Monatschr. für Geburtsk. Vol. x, p. 4.

‡ 'Der Gebärmutterkrebs.' Leipzig, 1858. P. 34 and following.

According to Klob,* the papillary cancroid growth that generally begins in the region of the os tincæ, or which arises as a pedunculated tumour from the labia uteri, and which is very often the initial form of the later medullary cancer, has sometimes been seen to originate in the cervical canal.

Spiegelberg,† says that cancer beginning from the glands of the cervical canal (alveolar cancer) is an occurrence so rare that Waldeyer examined only a single case, and he himself noticed only one.

Courty,‡ treating of the diagnosis of epithelioma of the cervical cavity, admits the difficulties of such diagnosis, but his process of overcoming these difficulties is not satisfactory. He does not seem to speak from his own experience.

Barnes § does not describe the cancer of the lining membrane of the cervix, but when treating of the fungosities, carnosities, and excrescences that accompany endometritis, and especially the cystic form, he opines that they may depend sometimes upon malignant disease of the lining membrane.

Schroeder ‖ describes, in a few words, only a single case, which he considers to be a very rare instance. "At the post-mortem examination of a woman who died after ovariotomy we found accidentally a medullary cancer of the superior part of the cervix uteri. A portion was softened and formed a cavity which was covered by the lining membrane of the cervical canal, this membrane being quite smooth. The vaginal portion presented no abnormity. The removed kystoma ovarii showed no trace of malignant disease, but one lymphatic gland in the broad ligament and the retro-peritoneal glands were in a state of cancerous degeneration."

* 'Pathol. Anatomie der Weibl. Sexualorg.' Wien, 1864. P. 175.

† Ibid.

‡ 'Traité Pratique des Maladies,' &c. Second edition. Paris, 1872. P. 1009.

§ 'A Clinical History,' &c. London, 1873. P. 545.

‖ 'Handbuch der Krankheiten der Weibl. Geschlechtsorg.' Leipzig, 1874. P. 270.

In my practice I have met more than once with cases of cancer in which the growth began upon the mucous membrane of the cervix uteri. Some of these cases were seen only in consultation, other patients called but once at my dispensary. I had, therefore, no opportunity of taking notes, and I must restrict myself to relating four cases lately observed.

Case 1.—In May last I examined Mrs. L—, an old and emaciated patient, who complained of uterine hæmorrhage lasting for some months, and alternating with watery discharge.

The uterus was movable and not particularly enlarged; the os was patulous and filled with a friable, placenta-like mass, which overspread the posterior lips; and the latter seemed to be the seat of degeneration, while the anterior lip was smooth and pale. The growth was easily and completely removed by Simon's scoop.* During the operation the mass was found arising from the posterior wall of the cervix, and like a polypus, to fill the cervical cavity.

When completely removed the posterior lip was found quite healthy, as the growth which covered it did not adhere to the surface. The microscopical examination of the mass revealed the characters of a papillary cancroid tumour.

I do not know if there was any relapse, because the lady died two months later of a cerebral apoplexy, and no post-mortem examination was made.

Similar cases have been published some years ago by Küchenmeister.†

Case 2.—Mrs. E. B—, æt. 48, the mother of two children, complained for many months of severe flooding, of fœtid discharge, of pains, &c.

When I first saw her (March 1874) there was already

* 'Beiträge zur Geburtshülfe.' Berlin, 1872. Vol. i, p. 17.

† 'Oesterreichische Zeitschrift für Prakt. Heilk.' 1868. No. 6.

great emaciation. The uterus was enlarged and immovable; through the fornix vaginæ the irregular, swollen, and hardened walls of the cervix were easy to be felt. The os was patulous and contained a mass which afforded all the characters of cancer; but the edges of the os uteri were soft, smooth, and without thickening. The woman was admitted into hospital and remained under my care for about four months. During this time the degeneration spread to the rest of the vaginal portion, to the vagina itself, and to the rectum.

At the post-mortem examination the vagina, the cervical canal, and the rectum formed only one large cavity, but the disease had spared the corpus uteri; it ended at the os internum. There was only a slight hypertrophy of the body of the womb.

There can be no doubt that the growth had in this instance begun in the cervix, and possibly in its lining membrane.

The following most interesting case, still under my care, proves that difficulties of diagnosis exist even when the cancer is already ulcerated, and that they may be overcome by a diagnosis per exclusionem:

Case 3.—About five years ago, Mrs. A—, æt. 36, the mother of three children, shortly after her last confinement began to complain of leucorrhœa and lumbar pains. For about eighteen months she suffered from menorrhagia, and sometimes, but very seldom, from severe flooding that occurred at the intermenstrual period. The discharge presently became very offensive and either watery or slightly tinged with blood. She complained of neuralgic pains in the pelvis that made her often pass sleepless nights. She has been treated by several medical men with injections and cauterizations, but she derived no benefit from the treatment.

I was called for the first time in December, 1873, when the two distinguished practitioners who had treated her before still differed upon the subject of diagnosis,

one considering it a case of metritis and endometritis chronica, the other holding it to be cancer of the womb.

Mrs. A— was apparently in perfect health, and her embonpoint had scarcely diminished, although she could not leave her bed for more than a few hours every day, and she suffered great pain. There was no tenderness of the abdomen; the uterus was enlarged and strongly anteverted, but still movable, while its passive movements were not very painful. An attentive bimanual examination proved that the enlargement was due especially to the cervix and affected both walls. The vaginal portion was shortened, the lips were smooth, the os was regular and did not admit the point of the index. The uterine sound was easily introduced and reached the os internum, causing only slight hæmorrhage and no pain; but we never succeeded in introducing it further, as repeated attempts were painful and caused severe hæmorrhage; we therefore deemed it safer not to insist upon further introduction.

Another peculiarity of the cervical canal, however, struck me from my first visit. A few millimeters above the os externum I found the whole canal greatly dilated. The sound could be turned in every direction in the cervical cavity, and thick pledgets of cotton wool, that passed the os only with difficulty, could be freely moved in the canal itself.

With the duck-bill speculum we succeeded easily in seeing the mouth of the womb; the mucous membrane covering the vaginal portion exhibited no abnormity. The discharge was pure blood, or a serous, fœtid fluid.

I declared at once the case to be cancer; the presence of other growths (fibrous tumour, sarcoma) being disproved by the absence of one or more circumscribed tumours, and by the swelling of the cervix being regular and affecting equally both walls. I could only hesitate between cancer of the neck and chronic metritis with endometritis.

Endometritis was rendered improbable by the following considerations:

a. The fœtid discharge may accompany endometritis when the muco-pus finds no exit, but it is at least very rare when the os is not too narrow, when there is no flexion, and when no other cause of retention exists.

b. The complete absence of mucus in the discharge. We know that in endometritis (villosa or cystica) there is always a hypertrophical state, and even a new formation of mucous glands, and that one of the peculiarities of this endometritis is the copious flow of mucus. The utter absence of this discharge made it highly probable that the lining membrane was either destroyed by ulceration, or its place taken by a morbid growth.

c. The enlargement of the cervical cavity, that could only be produced by a deeply exulcerating process.

To these three considerations we may perhaps add—

d. Ex juvantibus et nocentibus; in our case, ex non juvantibus. As treatment for endometritis had no effect on the patient, strongly astringent topical applications (nitrate of silver, tannic acid, &c.), having already been tried without success, I had further reason to suspect cancer.

For greater security I removed a fragment of the cervical wall with the sharp-edged scoop; but the first microscopical examination was unsatisfactory, owing perhaps to the exiguity of the part removed. We found only the characteristic reperta of granulations, namely, young connective tissue represented by soft and not well-marked fibrous structure with many spindle-cells; the greatest part of the microscopical object consisted of a large quantity of small, round granular cells; besides which we found, here and there, epithelial cylindrical scales in no great number and showing no regular arrangement.

I repeated the scooping a few days afterwards and removed larger fragments of tissue. The greater part of it presented the characters of the other portion; but the

cuts were more successful than the first time, as we found in every one of them a certain number of round, oval, or oblong spaces filled with epithelial cylindrical scales, arranged in a certain order. Evidently the growth could only be an epithelial cancer.

I should mention that in these microscopical examinations I was assisted by Drs. Menzel and Pertot.

The further course of the disease still more justified my diagnosis, and when I last examined Mrs. A—, on the 3rd of November, 1871, I found the uterus immovable; the posterior lip adhered to the wall of the vagina, whilst a thin edge of the anterior lip remained still free from degeneration. Severe flooding happened in the last weeks, the pains increased and the constitution of the patient was greatly impaired.

I may add another which may have some practical importance with respect to treatment.

Case 4.—Mrs. C—, æt. 36, has been confined five times. For some months before the date of my first visit she complained of hæmorrhage and watery discharge which was not offensive. She did not suffer pain, beyond a sensation of bearing down, and there was frequent need of passing water.

The uterus was movable and not enlarged; the lowest end of the vaginal portion had undergone an alteration that must be considered cancerous, there being a knotty, hard, irregular tumour that bled even at the slightest touch. Around and above the tumour I could feel the neck of the womb, which did not present the least alteration.

Some years ago I would not have hesitated to amputate the degenerated neck in the firm belief that the amputation would have succeeded in completely removing the morbid growth; but the poor results generally obtained by amputation of the cancerous neck, with exception of cases of cauliflower excrescence, made me prudent. The better to examine the growth I resolved to remove as

much as possible of it by the sharp-edged scoop. Should the degeneration prove, after scooping, not to be more extended than it appeared at first sight, nothing could be lost by performing amputation of the neck a few days later.

Accordingly I scooped off a great part of the tumour on the following day; it was removed with some difficulty owing to its hardened consistency. I found that by this operation I had opened a large cavity: it was the cervical canal, which was greatly dilated, and whose surface was hard, rigid, and ulcerated; the form of the cavity was irregular, and the infiltration of its inner surface extended to the os uteri internum.

The degeneration, which on the exterior part of the cervix had spread only a few millimeters beyond the os externum, had reached on its inner surface the os internum. If I had performed amputation, the wire or the knife would have cut into the cancerous tumour itself.

It is impossible to determine in this instance where the growth originated. It might have been in the cervical cavity; at least, to judge from the fact that the destructive process was far more advanced in the cervical canal than on the outer surface of the vaginal portion.

In every case the knowledge of the possibility that cancer of the neck may spread much higher on the lining membrane of the cervical cavity than on the exterior part of the neck teaches us, whenever amputation is thought possible, to examine and ascertain whether the cervical mucous membrane be not degenerated. Thus we support the views of Spiegelberg,* who considers amputation of the neck useless in all cases of cancer, with exception of pedunculated, cancroid, papillary tumours of the lips; and we oppose the opinion of Barnes,† who advises to remove the diseased portion, if a distinct neck be felt above it, and if the uterus continues to be movable.

* 'Archiv für Gynäkologie,' vol. v, p. 411 and following.

† L. c., p. 845.

The PRESIDENT remarked that Dr. Leibman had drawn attention to an interesting pathological subject. The object of his paper was to point out that authorities generally regarded the vaginal portions of the cervix as the seat of the beginning of cancer, whatever its form may be, but that he had observed cases in which the disease commenced in the cervical cavity, and he had been able to demonstrate the development of masses of epithelioma in that locality while the external labia were entirely unaffected. The author, however, did not seem to be conversant with the researches of Virchow and other pathologists, who had observed cancer originating both in the mucous membrane of the cervix and body of the womb, and had demonstrated the development of cancroid in the uterine walls themselves. Cancer beginning first in the fundus uteri was not uncommon. Sir James Simpson had written a paper on this subject, and he believed instances had been published by Dr. Barnes and others. He had in recollection more than one case where he had watched the progress of malignant disease in the body and fundus of the uterus, and where ulceration had made its way into the peritoneal cavity, with the cervix, to all appearance, scarcely at all involved. The difficulty of diagnosis in cases of cancer of the interior of the uterus was, no doubt, very great, and he knew no way of distinguishing a malignant growth situated in the fundus from the enlargements of a benign character, except by observing the nature of the discharges, the comparatively rapid growth, and the persistent pain associated with the former. In the treatment of epithelioma or other malignant diseases of the cervix he believed there was a general concurrence of opinion that when the tumour had a margin of sound tissue above it was desirable to amputate the cervix; but he had not yet made up his mind whether in worse cases, viz. where the disease had advanced above the line of demarcation between the cervix and body of the uterus and it was not possible to remove the whole morbid growth, it was desirable by operative procedure to remove a considerable portion by way of staying the progress and lessening the discharges. He thought this might be justifiable where the discharges and loss of blood were great and rapidly reducing the patient; but where the symptoms were less urgent his experience rather indicated abstinence from interference, as any but palliative measures had seemed to give an impetus to progress and to lead to more rapid development of the disease.

Dr. WILTSHIRE thought the "beginning of cancer" of singular importance to the profession as a matter of clinical as well as of pathological interest. It involves an important practical point, for it is obvious that if we can find out the beginning of the disease, and are thus enabled to treat early the implicated tissues, we may at least have the satisfaction of arresting the disease and that sometimes for a considerable period. Dr. Wiltshire men-

tioned two cases in illustration of his remarks. In both considerable benefit followed removal of such portions of the diseased tissues as could readily be attained. He had sometimes used chromic acid with considerable success. Some patients died with symptoms of Bright's disease, doubtless from implication of the ureters.

Dr. Heywood Smith, having made a few remarks on the far greater frequency with which cancer is found to be developed in the lips of the uterus than the fundus or body, and in the posterior lip of the two, proceeded to criticise the treatment with plugs of strong perchloride of iron, as had been recommended by Dr. Gibb. Since Dr. Gibb's article appeared he had tried the iron in many cases, and in a large proportion had seen, with some arrest of discharge, grave symptoms supervene—symptoms as of septicæmia in one case, the temperature being as high as 103° F. He laid stress on the rule that Dr. Barnes had insisted on, viz. that in all cases where practicable early removal should be had recourse to.

Dr. Rogers thought the paper useful. He thought bromine a very useful agent in treating these cases, but care was necessary, as the drug was such a powerful escharotic. Previous scraping might be beneficial.

Dr. Barnes, not having heard the paper, would limit his observations to one question of treatment referred to by the President. After the experience of quite a considerable number of operations he had arrived at the conclusion that the most effective as well as the most safe mode of dealing with cancer of the cervix uteri was by the galvanic cautery. If the diseased mass projected into the vagina in such a manner as to permit of being surrounded by a wire, it should be removed flush with the vaginal roof. There was rarely any serious bleeding, sometimes almost none; now and then, if a small artery spouted, the use of the porcelain cautery of the battery had effectually stopped it. In those cases in which the disease did not form a projecting mass the button galvanic cautery could be moved over the surface, destroying the necrosed surface. Comparatively healthy granulations commonly followed; the so-called cachexia disappeared for a time; there was almost always freedom from hæmorrhage for a time; and the general condition greatly improved. He had only known one casualty from the proceeding. A woman in St. Thomas's died, a few days after operation, from peritonitis. In all the other cases reprieve and benefit were experienced. In one case the subject had two pregnancies, and she was alive five years after operation, although the disease returned.

Dr. Wynn Williams remarked that the cancerous matter may be deposited in any of the tissues of the uterus, as in other parts of the body, also that different forms of cancer begin more

frequently in one structure than in the other; thus, the epithelial form commences in the skin and mucous membranes, whereas the medullary commences in the deeper-seated parts. So it is with the uterus; you may have cancerous deposit in any part of the uterine organs. He had seen in consultation three cases within the last twelve months where he diagnosed cancer of the fundus. He did not think that any one could well mistake the peculiar odour of a cancerous mass in its later stages for the odour of a disintegrating fibroid. In cancer of the fundus the greatest care should be taken in passing the sound, as a very eminent physician informed him on one occasion, whilst examining the uterus, he was unfortunate enough to pass the sound through the cancerous tissues into the peritoneum, the patient dying in a few hours of peritonitis. There can be no doubt that the cancerous mass, wherever situated, should be removed when practicable, either by the écraseur, or by the galvanic cautery as mentioned by Dr. Barnes; but supposing you do not remove the whole in this manner, are you to let the patient succumb to the disease? Certainly not. Where the uterus is movable and the surrounding tissues not infiltrated you should endeavour to destroy the remaining portion by injecting bromine into it, or applying it by means of cotton-wool and a cup. This I have done in several instances where the neck of the uterus has been removed and the actual cautery applied by other medical men with the result apparently of removing the whole of the disease; at any rate, six or seven years have elapsed and the disease has not reappeared.

Dr. Routh said the Society might recollect his (Dr. Routh) bringing the subject of uterine cancer before the Society several years ago, and subsequent experience had only confirmed the opinions then expressed. Indeed, one of those women, a case then believed to be hopeless, was, he had heard, still alive and in perfect health. He would first speak of the existence of cancer at the *fundus*. He did not believe it was quite so rare as had been stated, but it was, nevertheless, not frequently met with, and occasionally these cases were overlooked because mistaken for fibroids. Indeed, it was quite impossible in many cases to make a diagnosis, because the bloody or fœtid discharges were common to cancer and disorganising fibroids, and even fœtid discharges might occur without either disease; but if there was *marked pain*, especially as *at night*, which persisted for *any length of time*, and with menorrhagia, it was safer to put down the case as one of cancer. Perhaps some very rare forms of neuralgic uterus with menorrhagia might puzzle us occasionally, but the previous history generally cleared up the case. When the cancer affected the *cervix* exclusively, and always in cases of *papillary* or *epitheliomatous* cancer, it was the right thing to remove it at once. He had done so in several cases, and many had had no recurrence. He usually employed the ordinary wire

écraseur in preference to the electric cautery. First, it was much more easy of application, especially if the tumour was large or irregular, because the electric wire was so thin. Then, secondly, the electric wire often burnt a good way around the burning wire (sometimes as much as half an inch), and if the cervix had to be taken away high up there was fear of injuring the attachment of the vagina and uterus, and so getting into Douglas's space from the after-sloughing and producing death by peritonitis. But he (Dr. Routh) was not satisfied with leaving the case to heal at once after such excision. He waited four or five days, and then destroyed the surface by means of *bromine* or the *actual cautery*, again allowing the destruction to extend to part of the lining membrane of the uterus, if he had reason to fear it was diseased at all; and then he often expedited the healing process, especially by a solution of gastric juice, which had a marked effect in bringing about cicatrization. This he had proved often, not only in cancerous sores, but in others which turned out very obstinate and difficult of cure by other means. The marked difference between it and the *perchloride of iron* in healing power could be seen often if we dressed a wound half with the *iron* and half with gastric juice, and he was glad to find that Dr. Barnes confirmed the healing power of this agent. A relapse would not justify cessation from further interference. A second operation often proved effective when the first had failed. Lastly, in cases of *hopeless cancer* accompanied with fœtor and loss of blood, and when a patient was dying a miserable death, he thought we should interfere. Destruction of the ulcerating surface often stopped both and the general cachexia, and gave great relief, and the patient gained temporary restoration of health. When the hæmorrhage proceeded from the cavity he applied the *persulphate solution of iron* or the *perchloride* directly to the cavity on lint; and more lately, by means of a larger but similar instrument to that devised by Simpson for passing caustic into the womb, he introduced into the cavity, either after dilatation by sea tangle, or without if the opening was sufficiently patent, the *dry solid perchloride* with the best results. Sometimes the arrest of hæmorrhage was instantaneous, and seldom gave rise to any trouble. Once only he had seen some metritis which supervened and lasted three or four days. In any case he thought to leave even hopeless cases of uterine cancer to die a miserable death, and a pest to themselves and others, in our present state of knowledge, when so much temporary relief could be given, was perfectly unjustifiable.

Dr. Aveling stated that, after scooping or removing in any other way the cancerous structures, he had found *nitric acid* as efficacious as and more easily applied than the actual cautery.

Dr. Bloxam said that all present would agree that it was of the utmost importance in those cases which were beyond the

reach of operation to mitigate the suffering caused to patient and friends by the extremely fœtid discharges. He had heard bromide named, but not iodine nor iodofoom. The last was both a deodorant and an anodyne, and deserved more notice than it had yet received in this country. It could be applied in powder on wool, as a pessary with cocoa-butter, or in solution in glycerine.

Dr. Edis remarked that the object of Dr. Liebman's paper was to show that in the majority of cases of cancer of the uterus the disease commenced in the lining membrane of the cervix, and not primarily in the lower portion of the cervix itself. He (Dr. Edis), had long recognised the fact, and in place of amputating the cervix by means of the écraseur had been in the habit of employing the actual cautery in the form of a pear-shaped bulb to destroy the lining membrane and interior of the cervix, thus producing a conical cavity, the base representing the os uteri, which ultimately contracted, leaving merely a small aperture. In several instances this had been attended by marked success, the patient living for many years after the date of operation without any apparent return of the disease. The chief points in enabling us to arrive at a correct diagnosis as regards the fact of its being cancer were the almost undilatable condition of the cervix, the extreme tendency to bleed on the slightest touch, the fixidity of the mucous membrane covering the cervix, and the rapidity with which the disease extended.

Dr. J. Braxton Hicks said, with regard to the deep burning of the galvanic wire mentioned by Dr. Routh, this was obviated by making the battery more powerful and screwing up the wire quicker. The galvanic wire had one great advantage over the ordinary écraseur, it cut quite clean and knife-like, and was adaptable to the more sessile growths, whereas the écraseur drew in, and puckered up such forms, and brought away the softer portions gliding off the harder, which it left behind. This he had seen frequently. With regard to the practice advocated by most of the speakers, namely, removing those parts which we could, and destroying the rest by cautery, caustics, or styptics, or other plan, he coincided; indeed, these he had advocated in papers elsewhere and at this Society. It afforded much benefit and comfort to the patient, removing the cachexia very effectually till the return of the disease. This he had shown in a paper in the 'Guy's Reports' some years since, and concluded that the so-called malignant cachexia was owing, not to malignant diathesis, but to the absorption of the material from the malignant growth, prior to the existence of sloughing, though after breaking up occurred it was increased. Regarding the place of origin of malignant disease, he considered as the result of observation that it could spring from any tissue in the uterus, and then spread to other tissues; each fresh tissue affected would take on the action

more according to its own nature than of that from which it was influenced; for instance, that epithelioma would not communicate epithelioma to fibrous or connective tissue, but supposing epithelium was first affected and the malignant action extended to fibrous tissue, then rather that schirrus was formed, and so in connective tissue we get the infiltrating kind like that which occurs in the connecting tissue in the breast. However, this opened up the question as to the nature of malignant disease at large, which was too wide a subject for the present time, but he would say that he had seen on the confines of malignant disease of the cervix removed by amputation under the microscope, the mode of extension taking place in the nuclei of the fibre-cell enlarging, then dividing inside the old cell, bulging out its centre. In like manner, as he had pointed out in the 'Guy's Reports,' and again in the 'Pathological Transactions,' he had seen the nucleus in the wall of the blood-vessels enlarge, and then divide into two or three cells, bulging into the canal of the blood-vessels, giving it a bead-like appearance, but ultimately going on so far as to obstruct the current completely; and this would serve to explain why it was that sloughing so readily occurred in some of these growths.

Dr. Murray thought that the practical point and treatment of the case had been shown by Dr. Godson. For, if ever pregnancy had been discovered, surely it was still advisable to remove any growth which might, by increasing, actually prevent delivery at the full term of gestation, and possibly necessitate the Cæsarian operation. The risk of labour coming on after the removal of the growth ought not to overbalance the eventual probability of a far more serious operation to the mother. Dr. Murray quite agreed with the President as to the difficulty of diagnosis in cases of cancer of the fundus uteri and fibroid disease of the body of the uterus. He had lately seen a case where not until after death was it shown that true cancer of the fundus uteri existed, and death resulted from perforation of the uterine walls with hæmorrhage into the peritoneal cavity. During life none of the symptoms present more especially belonged to either cancer or fibroid disease.

CASE OF EPITHELIOMA OF THE CERVIX UTERI COMPLICATED WITH PREGNANCY; REMOVAL OF DISEASED PORTION; SUBSEQUENT DELIVERY OF A HEALTHY CHILD; RECURRING PREGNANCY.

By Charles T. Savory, M.D.
(Communicated by Clement Godson, M.D., &c.)

Early in October, 1870, I was first called to see J. W—, a married woman, æt. 35. She complained of severe pain in the lower abdomen, with a continuous sanious discharge from the vagina.

Upon examination I found a considerable epitheliomatous growth occupying the upper part of the vagina. I advised her immediate removal to St. Bartholomew's Hospital, her circumstances not being good. She was admitted into Martha Ward, on the 7th October, under the care of Dr. Greenhalgh.

For the following notes of her case whilst a patient there I am indebted to my friend Dr. Godson.

"On October 7th, 1870, J. W—, æt. 35, was admitted into St. Bartholomew's Hospital. Had been married sixteen years, had eight children, the first premature; all labours good and recoveries perfect, except after last child ten months ago, since which time has never been well. Three months after delivery she began to lose blood from the vagina, and this has continued, more or less, ever since; sometimes the discharge, instead of being sanious, is watery; it varies very much in character; sometimes is clotted, generally, especially of late, is very fœtid. She complains of shooting pains in left iliac fossa and sacral region, at times very severe."

"*Physical examination.*—Abdomen uniformly distended, dull on percussion almost up to umbilicus, and a globular

swelling to be felt; apparently uterus enlarged. Vagina capacious, lax and moist; upper part occupied by a large cauliflower growth, into which nearly the whole cervix has been transformed; just a margin of healthy tissue left.

"October 27th.—Growth removed by wire écraseur.

"The patient left the hospital on 4th December, parts quite healed, and free from discharge; she still complains of a good deal of pain of the old character."

I saw her again on 5th December. From the symptoms which she described I was induced to examine her, and satisfied myself that she was pregnant, the fœtal movements being quite perceptible. Bearing in mind the possibility of laceration and hæmorrhage during labour, I left word that I was to be sent for as soon as pains set in.

On the 12th January, 1871, I was summoned in haste.

On my arrival I found the child born and the placenta expelled; labour had been unusually rapid; there was neither hæmorrhage nor laceration; recovery was uninterruptedly good; the child (a female) appeared quite healthy, though rather small, and is alive and well at the present time. The patient considered that her labour was premature by about five or six weeks.

After this I lost sight of her till January, 1873, when I was in attendance upon her for two or three weeks. At that time she was in a very weak state, losing a considerable quantity of blood, and was altogether so feeble that I thought she would not again leave her room; although she stated that up to within a week or two she had not, since her last confinement, been obliged to lay up for a single day.

On examination I found a large increase of the epitheliomatous growth. I gave her a mixture containing tincture of the perchloride of iron, and injections of a solution of sulphate of iron. She improved so far as to be able to come down stairs and resume her household duties.

In the early part of the following June I was passing her house, when she called me in, and stated that she believed herself again pregnant.

Upon examination she appeared to me to be about seven months advanced; the fœtal heart was distinctly audible. I wrote to Dr. Godson and made him acquainted with her condition, expressing a wish that he should see her. He did so on the 20th June, and the best treatment to be adopted was discussed. It was proposed to induce labour as soon as it could be conveniently arranged.

However, pending this, on the 25th June, early in the morning, I was sent for hurriedly.

On my arrival I found the liquor amnii discharged, pains occurring every ten minutes or so, but scarcely any appreciable dilatation of the os. This state of things continued till 3 or 4 o'clock in the afternoon, when a rigor occurred. There was just sufficient dilatation to admit the tip of the fore finger, the head was-found to be presenting, the pains were recurring about every five minutes, and the pulse was increasing in frequency.

After consulting with my partner, Mr. Pettifer, it was agreed to attempt to dilate the uterine orifice with a view to turning. A small Barnes's bag was therefore introduced, then a larger, and finally the largest, on the withdrawal of which it was found practicable to insert the points of three fingers. Manual dilatation was attempted, and after persevering for a considerable time the hand was introduced, a foot seized and delivery effected by turning, about 9 o'clock. The child (a fairly large one) was dead, but not decomposed; no undue hæmorrhage occurred, and the placenta was soon naturally expelled. An opiate was administered, and the patient passed a pretty good night. For the first week she appeared to be going on well, took food, and had but little pain; the vagina was syringed with Condy's fluid.

After this time she began to get weaker, and died on the 8th July (thirteen days after delivery) apparently from sheer exhaustion. There were no appearances of peritonitis,

or any inflammatory condition, and scarcely any pain from first to last.

The case appears to me to present so many points of interest that I have been induced to bring it before the notice of this Society.

In the first place, it illustrates in a striking manner how much may be done to the neck of the womb without prejudice to the functions of that organ; for here not only was gestation uninterfered with, but after a period of two years the woman again conceived. By the timely removal of the growth she was enabled to pass two years of life in the full discharge of her domestic duties.

Again, had pregnancy been suspected on her admission into the hospital the operation would certainly not have been performed, and the question of Cæsarian section would, in all probability, have been raised.

As it happened, the course pursued was in every respect the best, for by it the mother's life was prolonged and that of the infant saved; indeed, the results were so entirely satisfactory as to render it worth while to consider whether, under similar circumstances, the same means might not with advantage be adopted.

Lastly, this case bears out most fully the adage so often quoted by the teachers of our profession, that nearly as "much may be learnt from our errors as from our successes." I have not, therefore, allowed the circumstance of (I cannot say the error, but) the pardonable omission in the diagnosis of this case by the overlooking of pregnancy, to deter me from bringing it before you.

Dr. Godson remembered the case well as one full of interest. The enlargement of the uterus was attributed to the disease supervening so immediately upon parturition. It showed how careful one should be not to be led away by a very prominent feature from thoroughly investigating every point. He believed that by overlooking pregnancy the woman was perhaps spared the perils attending Cæsarian section. It had been argued in

favour of this operation, that, in cases of cancer and mollities ossium, the mother's life could be but short and full of misery, while the child's might prove of value to the state. The fact of the birth resulting in a girl, he imagined, would not affect the argument in these days of women's suffrage, at least as far as certain enthusiasts were concerned. But he considered that the life of the child should never, under any circumstances, be placed in the same balance as that of the mother. This case showed how much might be done to the uterus and its appendages during pregnancy. Here the cervical portion had been removed at the fifth month, and in another case Mr. Spencer Wells had performed ovariotomy at the same period without abortion taking place. Another point was what should be done where no portion of the malignant growth was capable of being removed, as in this case, during the subsequent pregnancy? Dr. Godson had advised the insertion of sponge-tents in order to soften the tissues, and dilate the os, as soon as the seventh month was reached.

The PRESIDENT inquired whether it had been proposed to perform Cæsarian section, or making incisions in the cervix to facilitate the expulsion of the child.

Dr. MURRAY remarked that, even supposing pregnancy had been diagnosed, no harm had resulted from removing the mass.

Dr. J. BRAXTON HICKS remarked on the importance of having some rules for the management of cases of pregnancy in malignant disease of cervix. He thought it, however, almost impossible to obtain any. Each case must be judged of by itself. However, in considering whether we should induce labour, or leave the case for Cæsarian section at full time, a primary question would arise—Can the woman live till full term? Again, in determining in regard to the induction of labour is the disease limited to the os and lower cervix, or does it extend to the lower part of the body of the uterus, in such a way as to render delivery very difficult? If these are difficult to answer, a third may arise—Can we leave the induction to the viability of the child, or must we procure abortion? So much depended on the amount and position of the disease and the state of the patient that Dr. Hicks feared it would be difficult to lay down definite rules. He mentioned some of the cases which he had seen.

Dr. HEYWOOD SMITH said that, with regard to this case, the fortunate thing for the patient was that delivery did not happen soon after the operation for removal of the diseased cervix, for he was sure that the puerperal state was a very great element of additional danger. About three years ago he had a similar case under him in the hospital with advanced epithelioma of the anterior lip, with some growth also on the posterior. Labour pains set in soon after the fifth month, and with the help of Dr. Squarey the patient was put under chloroform, and he (Dr. H. Smith) was surprised at the ready way in which the os uteri dilated. The

delivery was effected by podalic version and the child born alive. Respiration was with difficulty established and the child lived about ten hours. It was then determined to remove the disease and the two lips of the uterus were removed by two separate applications of the écraseur ; the tissue cut through was apparently healthy. The patient, however, died on the fifth day. Dr. Heywood Smith was convinced that the better practice would have been to have postponed the operation until the puerperal state had been recovered from.

The PRESIDENT thought that one of the most interesting points in the paper was that the operation was performed without bringing on labour. It was supposed nothing ought to be done during pregnancy—not even a tooth extracted ; but this case proved otherwise.

Dr. SAVORY thanked the Society for the kind reception given to his paper.

The PRESIDENT then announced that a special discussion would take place at the next meeting on the Relation of Puerperal Fever to Infective Diseases and Pyæmia, the subject being introduced by Mr. Spencer Wells.

APRIL 7TH, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—81 Fellows and 42 visitors.

Books were presented by Dr. Neugebauer, Dr. J. Amann, Dr. John Williams, and the Royal College of Physicians.

The following gentlemen were admitted Fellows of the Society: Dr. Herbert Cooper; Dr. Aust Lawrence; and Mr. Fred. W. Salzmann; and the following were declared admitted: Dr. R. J. M. Coffin (Malta); Mr. C. J. Cullingworth (Manchester); Mr. Peter Broome Giles (Hereford); and Dr. Charles Liebmann (Trieste).

Edwin Hollings, L.R.C.P. Ed., was elected a Fellow of the Society.

The following gentlemen were proposed for election: Mr. Thomas Bailey (Godston); Dr. R. B. Cole (San Francisco); Dr. Rt. Cory; Dr. W. H. Drew; Dr. A. T. Gibbings; Dr. James Gray (Glasgow); and Mr. Edwin M. Sheldon.

ON THE RELATION OF PUERPERAL FEVER TO THE INFECTIVE DISEASES AND PYÆMIA.

By T. Spencer Wells, F.R.C.S.

In opening a discussion this evening on the relation of puerperal fever to the infective diseases which are among the most numerous class of cases treated by the physician and general practitioner, and to the varied forms of traumatic fever which, under the head of pyæmia, fall more frequently under the care of the operating surgeon than of the obstetrician, I wish particularly to impress upon all who honour me by their attention that my object is rather to elicit than to impart information—to call forth the stores of knowledge now latent in the Society—to ask for the results of your observation, rather than to attempt to add to the knowledge of the Fellows by any contribution of my own.

It has been the custom of the Society in whose room we meet to stimulate the Fellows to enrich the 'Transactions' by papers of original research and sterling value; to make this one of the chief objects of the Society; and, if not directly to discourage, certainly not to encourage, full or exhaustive discussions at the meetings. I have long felt, and have publicly expressed my conviction, that this course might be amended with great advantage to all medical societies, and to the advancement of medical science and practice; and I had some small share, in the earliest of the preliminary meetings of this Society—I may say even before the Society was constituted—in determining that in our 'Transactions' there should be a permanent record not only of the papers read before the Society, but of the discussions to which the papers gave rise. This custom has ever since been honoured in the observance, and the example is now being followed by other societies. If I am not greatly mistaken, the dis-

cussion on pyæmia in the seventh volume of the Transactions of the Clinical Society will be referred to hereafter with more interest and profit than any of the papers in that or in any of the earlier volumes of that society's Transactions; and the discussion on cancer in the twenty-fifth volume of the Transactions of the Pathological Society was most certainly as great an addition to the value of that volume as one could well imagine, until last night many of us heard the opening address of Dr. Bastian on a subject closely allied to some of the questions which I have to bring before you this evening, and the admirable speech of Dr. Sanderson in commencing the discussion, both full of deep thought, and eminently characteristic of the tendencies of our age and our nation to reject any theory which is not supported by facts, and to bring the highest developments of science to our help in the needs of daily life. These discussions lead to the almost painful reflection, how greatly would the value of the earlier volumes of the 'Medico-Chirurgical Transactions' have been increased if we had now not only a copy of the papers read, but a report of the comments they called forth from Baillie and Halford; from Cline, Cooper, and Abernethy; from Travers, Brodie, and Lawrence; from Bright and Addison. The marble busts of these great men of the past now surround us; but the workers of the present and the future can find no record of what they have said here, and can only regret that, while their form and features are preserved by the sculptor, their thoughts have not been embalmed by the reporter and the printer.

> "Who of us can tell
> What he had been, had Cadmus never taught
> To man the magic that embalms the thought?"

As I have reason to believe that the discussion this evening will be fully and accurately reported, and will be preserved in our Transactions, I must now ask you to pardon these few prefatory remarks, and also to forgive me if I venture to express the hope that, as the subject of

puerperal fever is not of less interest than that of cancer, is closely allied with pyæmia and with the relation of bacteria to contagious and infectious diseases, it will be debated with as much ability and as complete freedom from any other than purely scientific and truthful feeling, as have characterised the discussions at the Clinical and the Pathological Societies.

If you permit me to consider what I have so far said as introductory, and to commence the subject of puerperal fever now, I hope I shall not encroach upon your time beyond the fifteen minutes within which limit I have tried to condense what I wish to say.

In order that some definite direction might be taken in this inquiry, attention has been already publicly requested to six leading questions. The first, second, and third are so closely allied that I will, if you please, read them together now.

1. Is there any form of continued fever, communicated by contagion or infection, and occurring in connection with childbirth, which is distinctly caused by a special morbid poison, and as definite in its progress and the local lesions associated with it, as typhus or typhoid, scarlet fever, measles, or smallpox?

2. May all forms of puerperal fever be referred to attacks of some infective continued fever—as scarlet fever or measles—occurring in connection with childbirth, on the one hand; or, on the other, to some form of surgical fever, or to erysipelas, caused by or associated with changes in the uterus and neighbouring parts following the process of childbirth?

3. If all cases of contagious and infectious diseases which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?

In framing these questions, after searching for an accurate definition of the term puerperal fever, or for some short description of this as distinguished from other forms of continued fever, I have taken, as the most accu-

rate and comprehensive, the definition from the Nomenclature of Diseases drawn up by a Committee appointed by the London College of Physicians (for which definition I believe the committee are indebted to Dr. Arthur Farre). It is this: "A continued fever, communicable by contagion, occurring in connection with childbirth, and often associated with extensive local lesions, especially of the uterine system." To this definition this very important *note* is added: "In returning cases of puerperal fever, the more important local lesions, such as peritonitis, effusions into serous and synovial cavities, phlebitis, and diffuse suppuration, should be specified."

Here, then, we are led, on the authority of the most distinguished obstetric teacher of his day, supported by a committee appointed by the Royal College of Physicians of London, to the conclusion that in puerperal fever we have a contagious continued fever *often* associated with the important local lesions just enumerated—not *always*, but *often*. You may then, according to this definition, have this contagious fever without these local lesions. The poison may be so potent, or the dose so large, that it may kill before there is time for the development of the local lesion; or the dose may be so small, or the poison so feeble, that it only produces some transient elevation of temperature, some greater rate in pulse and respiration, some increased action of skin, kidneys, and bowels, and the morbid material is eliminated before any local lesion is established. But I must ask you to say if, in your experience, you ever saw such a case which could not, on careful inquiry, be traced to exposure of the patient to some one or other of the contagious or infectious fevers—to scarlet fever or diphtheria—to measles or smallpox? I need not remind you how these diseases are intensified or modified by the puerperal condition; and I proceed to ask if, in any case where puerperal fever could not be proved to be really scarlet fever, diphtheria, measles, or smallpox occurring in connection with childbirth, it was not a traumatic or surgical fever, erysipelas, pyæmia, or

septicæmia; the local lesions associated with the fever assuming rather a primary than a secondary importance in the chain of sequence. Time does not permit me to enter on the very important practical question as to the sole dependence of erysipelas upon a specific morbid poison. Whether this peculiar inflammation of the skin and cellular tissue, tending to spread indefinitely and preceded or accompanied by fever, can arise from the spontaneous generation in the human body of a poison communicable by contagion; or, whether a poison capable of producing erysipelas exists at all times in varying quantity somewhere, ready to increase and multiply under favorable conditions; or, whether (as some believe) erysipelas may arise independently of any poisonous influence from without, we need not inquire now. The important fact for us now is, that erysipelas often attacks the parts concerned in the process of childbirth, and that the fever which accompanies it, intensified by the puerperal condition, is a very fatal form of one of the diseases confounded together under the term puerperal fever. Set aside the infectious fevers and erysipelas occurring in connection with childbirth; and then we come to the local lesions associated with puerperal fever, and we ask what relations this fever and the local lesions bear to each other. Bruises or tears of the genital canal or perinæum; inflammation, and the production of pyrogenic liquids or solids, which may contain both bacteria and some poisonous material or particles which have the power of impregnation; diphtheritic exudation on the mucous membrane of the uterus and vagina, especially on the place of the separated placenta, and plugging of the lymphatic vessels with granular masses or colonies of bacteria or spheroids; the rapid development and growth of plant-life, and the disturbance of function and alteration of structure which must follow; does all this arise under perfect sanitary conditions, spontaneously, or from mere chemical decomposition, or only when some poisonous agent is introduced from without—the seed of some plant sown in a fruitful soil?

Again, supposing inflammation is set up in the uterus, in its veins and lymphatics; that the albuminoid secretion known as the lochial discharge contains pus; that pus, or putrid material, or organic germs, are found in the lymphatic vessels of the uterus and the subperitoneal cellular tissue, with diffuse peritonitis; that the blood in the uterine veins clots, softens, breaks up, is the seat of chemical and vital change, is detained in or near the pelvis, or is carried away to distant parts, or alters the composition and properties of all the blood in the body; that we have purulent infection, or pyæmia—putrid infection, or septicæmia; can all this arise in a healthy woman, placed in favorable conditions, if she be not exposed to some morbid poison? Is puerperal fever ever a simple traumatic fever modified by the puerperal condition, or does it always and necessarily depend on the action of a morbid poison? Or, let me put the question in another form. Did you ever see a case of puerperal fever which was not really either a case of scarlet fever or measles, or some such infectious or contagious fever, or erysipelas? or a traumatic fever caused by the bruising or tearing of the parts concerned in childbirth, and the changes in the blood-vessels, blood, and lymphatics following the injury? If you *have* seen such a case, then let us know something about the fever, its period of latency, its course and duration, and its termination; and especially tell us something about the poison which has been the cause of the fever.

Time does not allow me to do more than put the question. I await your reply, and pass on to the fourth, fifth, and sixth heads of our subject, which I will also read together. They are these:

4. Assuming that a form of continued fever—communicable by inoculation, contagion, or infection—does frequently occur in connection with childbirth, how can its spread in private and in hospital practice be most certainly prevented or checked?

5. What relation have bacteria and allied organic forms to the pyæmic process in the puerperal state?

6. What is the value of antiseptics in the prevention and treatment of puerperal fever?

And, to show the close connection of these with the previous questions, I must remind you that even those who admit that a tear of the perinæum, or a bruise of the vulva, by leading to inflammatory exudations around the vagina or uterus, or to clotting of blood in the veins, and consequent changes in the whole of the blood in the body, is quite sufficient to account for fever, which, aggravated by the puerperal condition, may lead to all the local lesions specified in the note to the official definition of puerperal fever—even those believers in the spontaneous or local origin of the fever, seeing the hundreds of cases where the injury is observed without the fever, for every one where the fever is observed, and the prevalence of the fever in certain seasons and districts, and in the practice of certain surgeons or midwives,—still freely admit that it is only under some endemic or epidemic condition, or as a result of contagion or infection, that the ordinary wounds, or bruises, or tears, inevitable during the process of parturition, and commonly free from any serious consequence, become in exceptional cases so deadly.

It is especially in this direction that the knowledge of our country Fellows may be of the greatest value to us. In the practice of a large hospital, or in private practice in a large city or a thickly populated district, it is impossible to say that a patient may not have been exposed to some contagious or infectious disease. But, when isolated cases arise in private practice in the country, where any source of poisoning can be readily traced, the accurate record of such cases, their origin and course, and their arrest or extension, may be of incalculable value. A country surgeon attends a man who has erysipelas after a broken arm. He also attends a healthy woman in an isolated cottage in a natural labour. There is no puerperal fever in the district, yet this woman dies of puerperal fever, and so do others attended about the same

time by the same surgeon. These women, in all probability, would have recovered in the ordinary course had not erysipelas occurred in the man who broke his arm. Such a history as this would have tenfold weight, as being free from numerous sources of fallacy and doubt, compared with any similar history in a large city.

And, again, we cannot consider the mode of preventing the spread of puerperal fever without examining into the part which bacteria and other organic forms may play either as poisons or as carriers of poisons. Here I must ask you to allow me to refer to a paper, which I read twelve years ago at Cambridge, "On the Causes of Excessive Mortality after Surgical Operations." It may be found in the second volume of the 'Medical Times and Gazette' for 1864. In that paper I gave some account of Pasteur's researches on fermentation, on the organised corpuscles in the air, on spontaneous generation, and on putrefaction; sketching rapidly the results of some of his researches, to show the influence of the germs of the lowest organisms present in the atmosphere, especially bacteria and vibrios, upon animal bodies in health and disease, and on our tissues during life and after death, and especially upon the development of epidemic and contagious diseases. I also showed, from the observations of Angus Smith, Chalvet, Eiselt, and Réveil, that germs may be often found in the air of crowded rooms and hospital-wards, which only require favorable conditions for their rapid development.

Now that the influence of bacteria is beginning to assume a more general importance in pathological investigation, I must remind you of the account, which was published twelve years ago, of what I even then thought Pasteur's discoveries were leading us to.

"Carrying on the analogy between puerperal fever and purulent infection in the various forms which contribute so large a share to the excessive mortality after surgical operations, and applying the knowledge, for which we are indebted to Pasteur, of the presence in the atmosphere of

organic germs which will grow, develop, and multiply, under favorable conditions, it is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or in pus, and that they so modify it as to convert it into a poison when absorbed—or that the germs after development, multiplication, and death, may form a putrid infecting matter—or that they may enter the blood and develop themselves, effecting in the process deadly changes in the circulating fluid. That these low forms of animal life may seriously affect the blood of the higher orders of animals is clearly proved by the recent researches of Davaine, who has furnished us with the first well-established example of a disease of the blood due to the presence of inferior beings which are capable of development and multiplication in the torrent of the circulation. These creatures (bacteria) differ from the whole class of infusoria which form in putrefied matter, as they disappear completely as soon as putrefaction of the blood commences. The bacteria are rapid consumers of oxygen, and when they exist in the blood they absorb the greater portion of the oxygen furnished by respiration, and thus hinder the combustion of all the effete and used-up substances which ought to be eliminated from the body. The blood, instead of nourishing the body, nourishes the parasites. Inoculation of animals with fresh blood which contains them leads to their development in the blood of the inoculated animal."

I may also say that, in the same paper, I gave some account of the relation of bacteria to splenic apoplexy in sheep; and referred to demonstrations of the presence of living germs in the air capable of reproducing contagious diseases, by Lemaire finding the achorion in the air which had passed over a scalp affected with favus, and by the experiments of Kennedy and Salisbury on the production of measles by the inoculation or inhalation of fungi given off from mouldy straw or linseed-meal. And then, in concluding that paper after recording proofs—1. That the injection of a certain quantity of pus into the

blood produces pyæmia and affections characterised by multiple abscesses; 2. That the injection of putrid matter produces septicæmia, or putrid infection, characterised by the symptoms of typhoid gastro-enteritis; 3. That the injection into the blood of the exudative materials in contagious diseases, as in glanders, produces in general contagious affections; and that, in all these cases, the introduction of the foreign substance or poison into the blood must be regarded as the origin of the disease—I went on to give some account of Polli's antiseptic treatment of these various forms of disease by sulphurous acid and the alkaline and earthy sulphites, showing how they arrest or prevent fermentation and putrefaction, and how it might not only be expected that the living fluids and tissues charged with the sulphites would resist the action of morbid poisons, and that there were proofs that, when the sulphites had been taken, they really altered the actions of pus upon the blood of a living animal, as well as that of putrid matters injected into the blood, and that of a virus distinctly contagious and not putrid.

This was in 1864. It was two years afterwards—towards the end of 1866—that Lister began to treat cases antiseptically in the Glasgow Infirmary, using carbolic acid rather than sulphurous acid or the sulphites, but with the express purpose of destroying the organic germs present in the air, or in any of the liquids or solid substances about the patient; or of protecting any wounded or injured part from the contact or development of the germs. How he has gone on gradually perfecting the details of the antiseptic system I need not describe to you; and the results are too well known to require more than the most passing allusion to the prevention of surgical fever, of pyæmia and septicæmia, the checking the spread of erysipelas after its importation into a ward, the lessening of mortality after both the greater and the more trifling operations, the saving of limbs after compound fractures, the healing of large burns, ulcers, or abscesses, and the general freedom of hospital-wards from noxious

odours and matters, and from the introduction of poison from the dead-house or dissecting-room, while the hospital atmosphere is not only purified for the patients, but for the surgeons and nurses.

If all this have been gained since 1866 in Glasgow and Edinburgh, and in other British and foreign hospitals, where the example has been more or less closely followed; if traumatic fever and pyæmia can be kept out of a surgical hospital, why should not puerperal fever be kept out of a lying-in hospital, or be prevented from spreading, if it have been accidentally imported? There has been a great outcry against lying-in hospitals of late; but I trust this Society may be able to guide the feeling rather in the direction of freeing them from puerperal fever than of destroying them. What has been done by reducing the size of a surgical hospital in Edinburgh and in London, the size of the wards, the number of beds in each ward, the number of attendants, and enforcing the utmost care in protecting the patient from any contagious or infectious influence, and in securing the greatest possible cleanliness in all things about her, in reducing the mortality after one of the most serious operations to which a human body can be subjected and still live,—is known to those who have watched the progress of ovariotomy. It is for you to say whether a similar enforcement of obedience to the laws of sanitary science, a more exact observation of the origin and progress of the many different conditions which have been classed together under the one head puerperal fever, and of the treatment called for by each condition; and a careful attention to the details of the antiseptic system, as it is brought more completely into accord with our daily increasing knowledge of the natural history of the lowest forms of organic life, shall or shall not be your rule and guide henceforth in your daily practice. What I have said in the very hasty and imperfect sketch it has been possible for me to offer in the few minutes you have so kindly accorded to me may soon pass out of your thoughts. But I am fully persuaded that

there are many here to-night who will be able to assure us that such a diminution of unnecessary or excessive mortality after childbirth may be hoped for as may relieve you and your successors from much anxiety and sorrow; and, while making your own lives more useful and happy, may raise still higher in national esteem the noble profession to which we should all be proud to belong.

A letter from Dr. J. Mathews Duncan was then read, regretting his inability to be present at the meeting. He stated that he had fully expressed his opinion on several of the topics of discussion in his Address to the British Medical Association at Norwich last year; yet he would be pleased to have an opportunity of saying more as to the conduct of practice. "It is, in my opinion, extremely unfortunate that midwives have been recently tried for what may be called puerperal fever manslaughter. I do not believe there were sufficient grounds for convicting and punishing Marsden. I have always practised, and intend to continue to do so, on the footing that care of the hands and clothes is a sufficient precaution for the safety of my patients. I have no belief whatever in any special virtue in going away, and I have no objection to my opinions being known. The 'Times'' assertion that it is necessary to give up practice for at least two or three months after having a puerperal fever case is too ludicrous."

Dr. Leishman.—Mr. President, in availing myself of the opportunity which your courteous invitation has permitted to me of taking part in this discussion—for I presume there can be little doubt that the remarks of to-night will more or less take the form of a discussion—I am a little taken aback in your having selected me as the first speaker after the distinguished essayist of this evening. I confess, however, that I have a peculiar satisfaction in being able to speak to-night on this subject. I may say I have in some measure a personal satisfaction in speaking, upon various grounds. I think I shall have the support and

sympathy of every person present whose duty it has been to teach the subject of obstetrics, and still more, perhaps, I may have the support of those who have had occasion to write upon this particular department of obstetrics, when I say that, when in their lectures they approach the subject of puerperal fever, they have felt that they were approaching a subject with which they were in a great measure incompetent to deal. It is not for me to explain, nor would I presume for a moment to indicate, the reasons of this in all their bearings; but I can well understand, and from experience I can assert, that the difference of opinion which has at all times existed on this subject has been the main reason of this confusion. I have a strong personal feeling in regard to this subject, because I have, to some limited extent, disseminated views which I now feel to have been erroneous; and I am, therefore, glad to have this opportunity, before such a distinguished body of my fellow practitioners, of disclaming, and in a sense abjuring, certain errors which I have had some share in disseminating among the juniors of the profession. I have regretted every day since I wrote on the subject that I had not more thoroughly investigated the evidence of the pyæmic source of puerperal fever; for the more I have done so, the more convinced have I become that, in a large proportion at all events of these cases, there is strong evidence to lead us to believe that they have their origin in pyæmic or septicæmic infection. I am not prepared to go the length which I believe many writers in Germany and some in this country have gone, in supposing that we are permitted to accept this pyæmic theory as the solution of all our difficulties; that by admitting pyæmia, or septicæmia, or ichoræmia, or whatever you choose to call it, as the cause of puerperal fever, we get rid of all our difficulties; and that for what we have hitherto considered *chaos*, we may in future read *cosmos*. I do not believe that it is so; for there are points of difficulty which must arise in the mind of every man of experience which have yet to be solved. I admit, and I should like to admit with all the emphasis possible on an

occasion like the present, that I believe the pyæmic or septicæmic origin of many cases of puerperal fever has been fully and thoroughly established; but I am not at all sure that this will account for all the cases that come before us in practice. There are cases in which a patient in the puerperal state unfortunately becomes the subject of diseases of specific origin. I may mention as an illustration scarlatina. A patient becomes infected with the specific poison of scarlatina. There is nothing in practice which we dread so much, and the result unfortunately proves that our dread is well founded. But, in the later history of such cases, I have had a difficulty in discovering any difference between the cases which we may suppose to have a septic origin and those which have proceeded from a specific poison. Again, there is another class of cases, in which it would appear as if the original symptoms were more those of a local inflammation, be it metritis or peritonitis, localised or general; and in all these instances, whatever the initiatory symptoms may have been, in so far as my experience enables me to form an opinion, I have again a difficulty in separating those cases, as far as the final symptoms are concerned, from those in which puerperal fever is dependent on pyæmia or septicæmia. These I take the liberty of presenting to the Society merely as the difficulties which have suggested themselves to my mind, having naturally, and as a matter of necessity, had the subject under consideration for a long time. I well know that in what I have written on the subject I have stated views which I am not prepared to support; I therefore began by frankly withdrawing something of what I had previously written; and perhaps what I now state may enable those members of the Society, if there be any such, who have done me the honour of perusing what I have published, to correct these views. There are other points which suggest difficulties. We have frequently been informed that these diseases are likely to be engendered by decomposing animal matters from whatever source the decomposition may originally arise. It has been very

commonly asserted, and rules in fact have been framed bearing upon the question, in those countries more particularly in which puerperal fever is most common, that students who dissect are liable to convey this infection. I have not the slightest idea of calling in question the accuracy of this assertion; but what I should like to point out is this, that if it were so certain and so frequent a method of communicating the disease as some would suppose, we should have far more puerperal fever in the practice of students who dissect. Then, again, if we are to assume this question of pyæmia as identical with puerperal fever, we know what pyæmia is in a surgical hospital. I presume it was the prevalence of pyæmia in surgical hospitals which led the late Sir James Simpson to write his celebrated papers on hospitalism. Pyæmia in a surgical hospital is a dreadful scourge. It may be conveyed by the surgeon; it may be conveyed by dressers, by nurses, by anybody; and beyond a doubt it may be conveyed, as Professor Lister has so clearly shown, through the atmosphere. I consider that that is a point that nowadays is demonstrated. Yet in surgical hospitals pyæmia is not such a scourge as puerperal fever is in lying-in hospitals. If you have the disease once existing in a lying-in hospital, you have it communicated with a frequency, with a fearful fatality in point of result, which it is appalling to contemplate; so much so, indeed, that I think I scarcely exaggerate matters when I say that, in some continental towns, the condition of a woman entering a lying-in hospital for attendance and care during the puerperal period is something like this: that she must be content to incur the risk of a patient who takes typhus fever, in the discharge of this purely physiological function. Now, this identity of pyæmia or septicæmia with puerperal fever may be established within certain limits, or it may not; but it appears to me that there is an intensity in the infection, that there is a peculiarity in the conditions, or there may be (as has often been said) in the woman in the puerperal state, a peculiar condition, which

renders her specially liable to impressions which we know very little about, that may account for all this. These are points which, in connection with those that have been formulated so kindly and ably for us by Mr. Spencer Wells, it appears to me should be taken under the consideration of the Society. I am unwilling to take up your time longer. There are many points on which I should like to speak; but I should be sorry to carry the forbearance of the Society even to the limits in point of time which you have laid down. I cannot, however, sit down without congratulating the Society as a body on the fact that this important subject, perhaps one of the most important of the present day, has been in this prominent manner brought under the notice of the Society, in the temperate and dispassionate manner that it has been to-night, rather inviting discussion than stating broad and distinct views with which we must agree, or from which we must be prepared absolutely to dissent. And I would also venture to predict that the result of the discussion which has been inaugurated to-night, arguing from the interest which it seems to have awakened in all quarters, will be a result which will be for good, for abiding good, on the future history of obstetrics.

Dr. Newman.—In rising to speak, I may say that I meet the requirements of your courteous invitation in two respects. I am a medical practitioner, and I have come from some distance—I cannot say with the express object of being present to-night—but, at all events, I am glad that the opportunity should offer itself to me of listening to the discussion this evening, on a subject of so much importance to us all. You will pardon me if, in speaking on the matter, I may seem to speak too egotistically. I simply say that, after some twenty years of tolerably hard country work, first in a village, then in a country town, in general practice and hospital practice, I have come to certain conclusions which may or may not be correct, and which I should have a difficulty in following

out one by one, giving you the arguments that have brought me to those conclusions. In taking the propositions which Mr. Spencer Wells has formulated for us this evening, I may say, in the first instance, that I should hold strongly that there is no such thing as a definite puerperal fever, so called. In the next place, I should say, with equal decision, that in a large number of cases of puerperal fever—I have traced it in my own personal experience, and have known it in the practice of other gentlemen with whom I happen to have been associated—there has been a distant, it may be yet a definite, link in the occurrence of possible transmission to the patient of some definite infecting poison. I cannot give a much more striking instance of it than this: Some years ago, I saw a lady who was exceedingly ill with puerperal fever. Two days after my visit she died. The opportunity was afforded me of close inquiry, I might almost say exhaustive inquiry, into the history of the few days or few weeks that preceded her delivery. It turned out that there had been a visit paid by her to the house of a neighbour in the same village, and a child in the house was at that moment suffering from scarlet fever. She knew nothing of it: she simply knew that the child was ill. She paid no attention to it, and, until the inquiry was pushed as closely as it could be, there had been no association in the mind of the patient, or in the mind of the medical attendant (for he came from a distance), or of any one connected with her, that that visit could have been the cause of the disease which led to the fatal issue. I should take that simply as a type, and say that my impression is that, in a large number of instances, of some of which I have notes, and of some none, the condition in question has its origin in direct or indirect communication with some infective process. There is another set of cases—and I am again drawing on my memory for a very clear one that I happen to have seen—where I believe that a local inflammatory mischief may unquestionably kill, by its production of a definite pyæmic condition, as cer-

tainly as it would kill in the instance of a surgical operation followed by pyæmia. I was asked some time ago to see a woman who had been suffering for two months. She was supposed to have been three or four or five months pregnant, and she had been suffering for two months from a certain amount of hæmorrhage. She had lost so much blood, and her general condition was so bad, that one felt that the only thing to offer her a reasonable chance was at once to empty the uterus. This was done by her own attendant at my suggestion, in the first instance by the dilatation of the os uteri, then by manual removal of the fœtus or ovum, and the secundines. Within three days she had rigor; and when I saw her subsequently she had all the symptoms of pyæmia. She had an abscess in the shoulder-joint, an abscess in the wrist, and not a few collections of matter in different portions of the body. That, again, I should take as a type of another set of cases, where I believe a morbid process, which we are satisfied to call puerperal fever for want of a better term, had its origin in a direct local lesion. That lesion may have been produced by enforced dilatation, and the forcible removal of the retained material, but still, there the condition was. I have further to say—and here I am referring to what has fallen from the preceding speaker—that one has very much to bear in mind that even in our present state, with fairly cared-for houses and the rest, there is not uncommonly such a condition of sewer air permeating the houses, that the general state of a parturient woman before her delivery is below par; hence the greater tendency on her part to absorb—if I may use the word—infective material, however it may be presented to her. I would further add that there can be very little question that the activity of the vital processes would certainly seem to be increased from the very earliest commencement of pregnancy to its close,—that the history of every pregnant woman must be understood by a no far-fetched analogy, to have a material influence upon the question before us. Given a woman in whom all the

processes, nervous, vital, circulatory, mental, if you like, are materially excited and altered from their condition of reasonable health; and I should maintain that in that phase alone we have much to look for in the way of an explanation of the reasons why poisons that run a different course, or at all events a slower course, under more ordinary states of everyday life, run, when they have to deal with a parturient woman, a course of far more severity and far greater rapidity, and, unhappily, far greater fatality. One more remark, and I have done. It seems that one should take into consideration, at all events that one should not hastily dismiss, the mental conditions which not uncommonly associate themselves with pregnancy. On this ground it happened to me some years ago to tabulate—I never did anything more than tabulate—a certain number of cases of puerperal fever that had occurred to my own knowledge. I think I may say that in a good number of them there were, at all events, the elements of distinct mental disturbance. I do not refer to mental disturbance in the sense of insanity, but in the sense of distressing circumstances, of the condition of pregnancy being a result of seduction, and many other reasons which will occur to gentlemen present. These do unquestionably seem to me to play a material part, at all events, in predisposing the system to the virulent development of septic poisons, however they happen to be introduced. I have to apologise to the Society for the very imperfect manner in which I have spoken. I began by saying that I would simply give my own personal impressions. I ask the Fellows to be good enough to accept them, and to give to them the very small value that they actually possess. I had not the intention, when I thought of coming to the Society, of joining in any way in the discussion; and I may almost say that, if I had not had a direct personal invitation, I should hardly have dared to do so.

Dr. Braxton Hicks.—I presume that by the discussion

in which we are engaged we shall not be able to clear up all the ambiguities and uncertainties with which this subject is surrounded. I take it that the principal advantage of such meetings is that the current opinion of observers may be brought out, and thus a certain impetus is given to the progress of knowledge, which would not have taken place, had each speaker waited to mature his opinions. Hence it follows that opinions expressed under such circumstances have more or less the disadvantage of imperfection, though it may be that the hints thrown out by the various speakers will stimulate thought in others, and, indeed, in themselves. The difficulties of this subject have been somewhat retarded by two means: 1st, most of the older observations were made in hospitals, and thus, if we admit that the disease is contagious, the character of the cases in each epidemic is similar. Thus, some have said puerperal fever was erysipelas, diphtheria, &c. Another retardation arose from the very means by which information was obtained; namely, from the *post-mortem* room. For, according as one appearance was more prominent, so it was considered the essence of the complaint; thus peritonitis, enteritis, phlebitis, &c., were considered the real disease, instead of being looked to as effect. Not that I would underrate the value of these investigations, carried on as they were at much personal risk, but to point out that these opinions prevented the observer from looking in the real direction, namely, to the clinical facts, derived not only from hospital, but from all the variable conditions of home attendance. However, the general *post-mortem* appearances having now been ascertained and well known, it is to the clinical study of the disease I would now urge the attention of obstetricians, leaving the inquiry as the exact nature of the poison as a separate one, simultaneously carried on, but still separately. By this means we shall proceed with more distinctness of purpose. I have already, in a paper called a "Contribution to the Knowledge of Puerperal Diseases," endeavoured to show the clinical aspect of the

conditions classed under the general head of "Puerperal Fever," taken from the various circumstances of home attendance in all classes of the community. I would not again detain the attention of this Society by alluding to the facts there shown, did I not consider them to have a great bearing on the subject of the paper of this evening, and that from further inquiries, the results there set forth may be taken as a fair average of practice as seen in consulting practice. There is no doubt but that they represent the severer cases; but I must here point out an error into which many fall in estimating the extent of the influence of circumstances on puerperal women by the death rate. It is not alone by the death rate we should judge; I would say, broadly, that where one dies, three or more are retarded in their recovery by either a more or less mild state of fever, or by the secondary effects well known to us, as cellutitis, phlegmasia, &c. Not till these also are calculated in can we recognise fully any influence brought to bear on the puerperal women. Now, the cases brought forward in that paper were those of simple labour: I did not include any about which I had made no inquiry as to the surroundings, either those which existed before or after. I found that, out of 89·68 had been connected in some kind or another with animal poisons, more than three fourths. Of these 68, more than half, namely, 37, had been connected with scarlatina in one way or another. Amongst the remainder, erysipelas, diphtheria, and offensive state of the discharges were prominent; of the remaining 21 I could trace no definite history in regard to zymotic diseases; but it would be difficult to say how many may not have been influenced by them, seeing it is hard to tell in many cases of zymotic diseases whence the source of infection had come. Some undoubtedly had been exposed to mental depression or excitement, were in low condition of health, or otherwise in conditions not favorable to recovery from any great disturbance. But of the first class it may be asked, Are these really cases of puerperal fever? Are they not properly cases of puer-

peral scarlatina, diphtheria, erysipelas, and so on? Let us, then, for the moment exclude them; though in doing so we are excluding three fourths what have been received as cases of puerperal fever; at any rate, with the exception of the well-marked scarlatina cases, let us examine the remaining 21. Shall we exclude or include those of them which have been brought on by sudden mental disturbances, as fright, annoyance, and anxiety? They probably amount to a fourth. If we exclude them, we shall be excluding cases like those which have been called "puerperal fever," and yet unless these influences generate the fever, we must exclude them. Again, some four of the 15 left were ill before labour set in, and these can scarcely be looked upon as cases of puerperal fever. Some of the yet remaining 11 possibly may have had an influence from a zymotic disease, as I have already hinted. Some were of traumatic origin, not toxæmic; and, allowing three for these, we reduce the total of unexplained origin to eight. Now, I would ask, Where is the proof of the existence of a separate entity such as is ordinarily understood by puerperal fever? and close upon this result, I maintain, will be arrived at if you take any hundred of cases. But when you come to examine the symptoms belonging to the various classes, you will find that they all belong to the same class; and if you, as I did for a moment, exclude those cases influenced by zymotic diseases, you are met by this difficulty: those without any specific symptom, as rash, &c., showed symptoms most typical of the so-called puerperal fever; that the specific symptoms were in all grades of proportions in the several cases; and that, generally speaking, the less the specific signs showed themselves, the more tendency there was to malignancy. This fact is well shown in hospitals where, erysipelas being the primary cause, the disease which followed would be more like to malignant puerperal fever, and less like erysipelas. In the paper alluded to many cases may be found showing this. I may venture to repeat one. A woman was taken with the most malig-

nant form of puerperal fever, and died about the third day. I could find no history of scarlet fever previously; but, in a few days, two of her children had malignant scarlet fever and died. Since my paper I saw the following case: a lady had been delivered twelve hours, when feverish symptoms commenced; a pile already inflamed became very tender and painful; from this part a blush spread something like erythema, but without any defined edge, and spread over her back. About the third day arthritic pains and swelling commenced, with delirium; these symptoms increased in intensity, and she died in great agony on the fifth day. As the symptoms began so soon after delivery, I could not help suspecting that it had its origin from without, as the symptoms arising from decomposing secretions commence from the third to the fifth day. I could not find that she had been exposed to any exanthem. The medical man had not seen a case for two or three months. The nurse was apparently free. I, however, told the medical man I thought he would have evidence in the children. In about a week the eldest child had scarlet fever, and rapidly died. The second was then attacked, and shortly died. Subsequently the baby was attacked, but recovered. In neither mother was there any specific sign of scarlatina. Numerous cases I have seen where scarlatina was in the house, and the mother had puerperal fever without any rash. In some of the other cases there were signs of a zymotic disease, yet they would have been overlooked had not care been taken, so masked were they by the general condition. But some have rejoined, "I have attended cases of scarlatina and delivered women constantly, and that without ill results." I would answer, Even so. But this is no proof that the influence of scarlatina is not detrimental. It is not every woman that is exposed to scarlatina who contracts it; on the contrary, most have already had it, and thus are less liable to catch it. There would be but few married women who would be susceptible to it. It would be an interesting point to know whether those seriously

affected by scarlatina have had it in former years. I have not sufficient information as yet. But, as I have said before, the death rate must not be the criterion of influence. We must watch the patient for a month. It is seldom but what we shall find a bad getting up in one who has been exposed to scarlatina just before or during her labour. An apparent contradiction to the influence of scarlatina has been instanced by the fact that sometimes, during an epidemic of scarlatina in a town, the lying-in hospital is free from puerperal fever. This is no proof. The only proof reliable is to be able to say that frequently cases of scarlatina have been introduced into lying-in wards, and no ill effect followed. The same answer applies to erysipelas. But there is another point which cannot be left out of consideration, viz., that violent mental emotions also are followed by symptoms precisely similar to those which follow zymotic influence, or the existence of putrid discharges. In fact, looking over the whole cases, we are, at least at present, unable to distinguish as a class the one from the other, excepting where the specific symptoms are superadded. To what conclusions, therefore, are we led? In some cases, no doubt, some other medium must be added, such as decomposing "sepsis," or the living bacteria, or some material which, mixing with the discharges in the uterine cavity, are absorbed into the system. It is difficult, in our present state of knowledge, to assign the proper value of this influence. That decomposing matter does cause these symptoms can readily be proved, but whether it acts through living or decomposing material, there is no evidence sufficient to permit us to argue. One thing seems to militate against the notion that it is the bacteria, which, in many cases, accompany the absorption of offensive discharges, namely, that if we wash out the uterus, the symptoms very rapidly subside; in twenty-four hours I have seen them pass mainly off. If living growths were going on, one would scarcely expect so rapid a subsidence; whatever may be the exact nature of the

poison, in our present knowledge I do not consider we are justified in basing any line of practice upon it; certainly not in allowing our clinical inquiries to receive any bias.

That the puerperal woman is, by the means of these various factors, brought into a state which we recognise as blood deterioration or disturbance, so that either she dies rapidly, or that, if this event be postponed, the processes required for repair after labour are so perverted as to be accompanied by inflammation extending to peritoneum, or producing effusions which often tend to suppuration; or to the uterine veins, producing plastic plugging and its consequences; or the blood assumes a tendency to coagulate, and thus fibrinous deposits form in the vessels; in fact, to all the secondary troubles well known to us all, as the results of the primary blood disturbance, but considered formerly as the principal condition. Such is the liability of the puerperal women to these deteriorating influences, that I think we may generally trace nearly every ill-getting-up to some depressing or disturbing influence, if we take the trouble to look for it. Whether the blood-conditions set up in the puerperal woman are similar to the so-called pyæmia, such as is observed in men and non-pregnant women, is not quite certain. That they seem to differ rather in intensity than in quality I think is generally admitted. We can readily understand the intensification when we consider the altered condition of the blood in pregnant women; and the ease with which the nervous system is perturbed; intensified by the large surface there is exposed, and the greater facilities for absorption there exist in the anatomy of the uterus, compared with the conditions found in the male. The position of the question appears to me this: admitting that various circumstances can set up a malignant fever in the puerperal woman, has this fever so set up a permanent character capable of being communicated to other pregnant women? Looking to the cases as they occur in hospitals, there seems some reason to think there

may be such a condition. Looking to private practice, one would also say so too, did we not see that apparently, when it spread to non-puerperal persons, it became reconverted to the zymotic form from which it had sprung. No doubt many difficulties attend this point of the inquiry; and to it attention should be particularly directed. However, without going so far as this, one may fairly say that a zymotic disease, if not modified in its true nature, is altered as to the usual character of the symptoms, assuming more the kind which have generally been called "malignant" in the non-puerperal person; and this tendency to change its character is more noticeable the nearer the patient has approached the full term of pregnancy; whether this depends on the changed condition of the system, or the greater patency of the lymph-spaces and veins, so as to increase the quantity absorbed, is not very clear. Seeing the many sources of contagion that surround all of us, I think that the notion that the aggregation of a number of puerperal cases can set up, *ab initio*, a puerperal fever has no resting-place. The aggregation unquestionably increases the number of chances of the introduction of some zymotic disease, while the exposure of a number to the disease will necessarily increase the number of persons affected. All aggregations do this. The same rule applies to surgical wards, with regard to erysipelas. While, therefore, I agree that aggregation tends to spread the disease, I am doubtful whether it sets it up, if cleanliness is carried out properly. But when once an exanthem has been introduced then its eradication is by no means easy, and the constant recurrence of cases, without complete purification, may fairly, to my mind, justify many of the remarks which were called, perhaps not quite correctly, "Hospitalism." There is only one other point I wish to allude to: this is respecting the contagious nature of the conditions grouped together as puerperal fever. That the majority are contagious to puerperal women I have no doubt; whether all are so I am uncertain. I am inclined to think that those forms

derived from the zymotic diseases are the most so; those from the self-generated kinds the least so. Some few seem not at all contagious. I remember a student attending a woman with puerperal fever for some days; during this time he attended some twenty cases of midwifery. None of these were ill after. A second instance occurred, before the nature of the case had been made out; upwards of twenty were attended without ill-result. However, I wish I could say this of all. I have had lists not unfrequently given me by practitioners, where the effects from a smaller series were very differently shown. I saw, a short time since, a most malignant form coming on apparently from mental distress. A coachman's wife, just after labour, heard that her husband had to leave his place. She was much affected, became violently maniacal for a few hours, and then subsided into the usual condition of malignant puerperal fever, of which she died in a few days. The medical man attending her told me he attended another woman in a day or two after, and she died in a rapid manner. But, surrounded as we all are by contagium, it is very difficult to say how far any case is free from zymotic influence.

Mr. Jonathan Hutchinson.—I, of course, have nothing to do with obstetrics, or with puerperal fever, but I believe that the subject is one which has its analogies in general surgical practice, and one of the most interesting points in Mr. Spencer Wells' address was the manner in which he traced that analogy. If I correctly understood his views, he seemed to place puerperal fever almost exactly in the position in which we place the several kinds of maladies from which our patients suffer after operations. He would say that there is no such thing as a specific poison which can produce any fever that should be known as "puerperal fever," but that a number of maladies induced by various causes have been grouped together under that name, and I thought that he very ably classified these according to the several causes which produced

them. I hope the time will come when he will extend his unwillingness to use such a vague term as "puerperal fever" to another which, I think, he accidentally used, "surgical fever." For, if "puerperal fever" be absurd, I contend that "surgical fever" still more leads us aside from the consideration of the true causes of the maladies in question.

If I may be permitted, I should like to say a few words on the great importance of attempting to define as precisely as we can the terms we use. I think we should much more easily get to conclusions, and find a much greater unanimity of opinion, if we did take these several terms, septicæmia, pyæmia, specific disease, specific fevers, and try and attach to them some definite meanings. Although it may seem presumptuous, I will state what my own opinions are upon these subjects, with great deference to the Society, and hoping to hear my views criticised and set right if they seem to be erroneous. I will speak first with regard to erysipelas, a disease which is of great importance in reference to puerperal fever, having the most of proof concerning it, that the contagion from it is one which is potent in the induction of the local inflammation which produces puerperal fever. I express, in the most unqualified terms, my belief that erysipelas is not a specific fever, that it is only a local form of inflammation, that the local form of inflammation may vary in intensity, may vary in duration, may be induced by many different causes, may undoubtedly be produced by contagion from the secretion from an erysipelatous patient, but may also be produced by other causes; and that the pyrexial symptoms and general disturbance are secondary to the local inflammation, and are proportionate to it. I have expressed this opinion on many occasions. It is the opinion, I believe, originally stated by Dr. Higginbotham of Nottingham, and I regret that in the majority of our systematic works their authors, without defending the position of erysipelas to rank as a specific disease, still so define it, and still speak of it as if it were a disease which has a stage

of incubation. I deny that it has any incubation stage, and assert that it has only one of commencing outbreak. There is a day or two when the patient is ill, the disease wants a little time to develop, but there is no true stage of incubation. We know that when it arises from contagion it will develop in a day or two after the virus being applied, or even within twenty-four hours, and that is fatal to its claim to rank as a disease due to a specific poison. Then we never see it prevail symmetrically in the two halves of the body, as it certainly would do if it were due to the introduction of any specific germs, which would develop in the blood in the same manner as we know the specific poisons of smallpox, scarlet fever, and measles do. I assert next that erysipelas may be checked at any stage; that appropriate treatment will stop it at a very early stage in a manner which would be utterly impossible if you were dealing with a specific fever. I next assert, that this will apply to stages of inflammation of the uterus and the uterine appendages which are due to erysipelatous contagion; that they are not to be ranked as specific fevers, but as things which may be put an end to by appropriate treatment at any stage. Dr. Braxton Hicks has just insisted, no doubt as the result of practical experience, on the importance of washing out the uterus; implying that the thing is curable, and does not run through any definite stages. Having made this assertion respecting erysipelas, that it is to be defined as a local inflammation which has special peculiarities that are capable of pretty easy definition, but having no true analogy to specific fevers, I must attempt to give some definition to the term septicæmia. Now, for that we have two meanings afloat in the professional mind. One is that it is due to some poison which is introduced into the patient's blood from without; that the poison then grows, spreads, germinates in the blood, and produces the symptoms. Now, what I would like to suggest is this, that the term septicæmia ought to be applied to the results of poisoning of blood induced by the inflammation of the patient's own tissues. I admit

that every now and then in connection with a poisoned wound the phenomena of septicæmia follow. To illustrate what I mean, I will mention two cases. I once amputated the finger of a professional friend on account of gangrene, the result of acute inflammation, owing to a poisoned wound. He had scratched his finger on a piece of carious bone. He had thought little of it at first, but acute inflammation had followed, had passed into gangrene, and I had to remove the finger at the joint. He was very ill at the time. He was in a state of septicæmia, the finger having inflamed and poisoned him. My own belief is that the character of the poison which was originally introduced was really of no importance; that there was something special in the state of the patient's health which permitted the gangrenous inflammation. The gangrene was the result of inflammation, and the constitutional symptoms were the result of the blood continuing its circulation through a portion of tissue which had passed into gangrene. I once amputated a foot on account of dry gangrene in a case in which I knew that the man's femoral artery was occluded. The gangrene was continually spreading, and we were obliged to remove it. Fearing gangrene of the stump, and knowing that there was no femoral artery, I amputated just below the knee. The leg was in a perfect state of health in that part; but within twenty-four hours of the amputation, in connection, no doubt, with the deficient supply of blood, the stump passed into gangrene, which spread rapidly to the thigh. The stump became dusky, and the patient was sick; the pulse rose to 160; he was feverish, had a dry tongue, and, in fact, was in a condition evidencing acute septicæmia. He was being poisoned, according to the way in which I would like to use this term, by the absorption of fluids from his gangrenous stump. The blood was going into it and back again, and receiving from it the fluids which were poisoning the body. The man would certainly have died if we had not adopted other measures. I amputated again, at once, just below the hip-joint, at a part where the vascular

supply was better. He was extremely ill at the time, but, contrary to my expectation, he made a good recovery. I quote the case as proving that the removal of the source of the poison was efficient in removing the septicæmic symptoms. I may also mention the case of a gentleman who had insured his life in the Accident Insurance Office. He had slipped in the street, and dislocated his thumb (a compound dislocation), and he died some six or eight days afterwards, with symptoms of septicæmia, vomiting, a rapid pulse, dry tongue, and shivering. His hand had passed into a state of gangrene. There was a long legal dispute as to whether he died of the accident, or of something in connection with his general health. I mention this case in connection with the others, as an illustration of what I believe, that it was the gangrene of the hand that poisoned the patient. I mentioned just now the case of a medical man who poisoned his finger, and in whom gangrene followed, and he had some of the symptoms of septicæmic poisoning. I wanted to confute the prevalent idea that some morbid matter from without is taken into the blood in every case in which septicæmic symptoms are present, and that that matter is the cause of the symptoms. In the next case in which I had to remove a finger for gangrene was a somewhat analogous one, but with an important difference. It was that of a poor seamstress who had no poisoned wound whatever. A prick of a clean needle on the finger had led to gangrene, and she had a similar train of symptoms. We very often get symptoms of septicæmia in cases in which there has been no possibility of their being produced by morbid poison from without; and it is quite impossible to distinguish between the cases in which gangrene follows an injury in a more or less unhealthy person without any introduction of morbid poison, and the cases in which inflammation follows when there has been some possible introduction of a small amount of morbid poison. I admit the influence of morbid matter as an irritant in setting up inflammation; but what I wish to state is, that I believe the stage of

gangrenous inflammation of the part is one which is essential to septicæmia, and that it is possible for this condition to occur without any local poison having been introduced from without. So much, then, for septicæmia. I think we should do well to keep this term quite distinct from that of pyæmia, and to believe that in septicæmia there are no emboli. Next, upon the yet more important subject of pyæmia, I have to say that I think we should give a little more attention to the opinions of our forefathers; and that I believe that no one could get more information respecting it from modern writers than from the original papers which drew attention to the subject, by the late Mr. Arnott, written some forty-five years ago. Next in importance to the papers of Mr. Arnott I rank those of Dr. Robert Lee: a series of admirable reports upon puerperal fever, a perfect mine of correct information upon that subject. If we studied those papers in the 'Medico-Chirurgical Transactions,' I feel certain that we should not have afloat the very erroneous opinions respecting pyæmia which have gained admission into some of our most important text-books, very much to the regret of many. The great heresy that at present prevails respecting pyæmia (at least, according to my belief) is, that it is possible for pyæmia to be induced by the introduction of a specific poison into the blood. I no more believe that than I believe septicæmia is from without. Pyæmia I hold, is produced by an inflammation of the patient's own tissues. What we call pyæmia, in all its more typical forms, is due to phlebitis. The old-fashioned notion of Mr. Arnott and Dr. Lee, that it is a poisoning of blood caused by inflammation of the veins is, I cannot doubt, quite correct. I regretted that Mr. Spencer Wells was a little vague upon that point. He spoke of coagulation of the blood, and the breaking-up of the coagula. Now, in spite of experiments upon the lower animals telling us of the difficulty of making veins inflame, I hold that any one who goes into the *post-mortem* room and examines the veins in cases of pyæmia will have no doubt

that the veins do inflame. And as to our being confuted by the fact that we have cases of phlebitis without pyæmia, that there may be coagulation of blood in the veins—solid masses in varicose veins, for instance—and yet no fear of pyæmia, I fail to see the relevancy of the objection. The things are totally different; but that there is a suppurative and gangrenous phlebitis in which the veins become full of pus, in which puro-lymph adheres to their ulcerated lining membranes, I hold to be thoroughly established by the records of surgery and by daily experience. It is not the breaking-down of blood-clot; it is ulcerative and suppurative destruction of the vein itself. I could produce illustrations to-night, showing this in various parts in the most typical cases of pyæmia. I do not say that the term pyæmia is to be restricted to these cases. I think it is quite possible that there are other cases in which multiple abscesses are formed, in which there is no phlebitis; but, I believe, we shall proceed on a safe basis of classification if we at any rate accept these most definite forms of pyæmia which are due to inflammation of the veins as the most important group in connection with the disease under discussion. Now, in what has been known as puerperal fever I have no doubt that phenomena precisely analogous to pyæmia on the one hand and septicæmia on the other will find their respective places.

Dr. Richardson.—If Mr. Spencer Wells succeeds in producing unanimity of opinion on the subject of puerperal fever, he may congratulate himself on achieving a success second only to his great achievements in surgery. I believe we are unanimous in the use of the term; but perhaps no two of us are agreed as to the clinical applicability of it when we meet with cases of puerperal fever. The view I would start with in regard to this subject rests with the condition of the woman after delivery. She is at that time, physiologically, in a peculiar position. First, her blood is in a peculiar condition. That colloidal fluid which,

separated into a solid form, we call fibrin, is sometimes in excess, from three to six or seven, and (as I once found on a direct analysis) eight parts in a thousand; so that the fibrine is at this moment in trembling equilibrium, ready on the slightest possible disturbance to be precipitated. Then there is a diminution of salts in the blood, a condition again favorable to the precipitation of the colloidal fibrin. Again, this woman has been for a time supplying to the child a mass of blood from her own body which has now stopped, so that practically she is in the condition of a person who has lost a limb or other portion of the body. She is also in an enfeebled nervous condition; she has been supplying from her own potential energy that nervous power which has been expended in the movements of the fœtus, and now that has stopped and she is suffering from the nervous reaction which comes on when that motion is suddenly arrested. She is, therefore, in the exact condition for a series of changes which must necessarily be febrile in character. I think we must bear in mind this position of women physiologically, before we enter upon the subject at all. To these facts we must add those which we have reason to learn in regard to hereditary qualities. We must accept the fact that there are a considerable number of women who are hereditarily predisposed to particular diseases, and we cannot except those in puerperal fever from this position. We come to consider the woman in this state, and we study the series of questions which Mr. Spencer Wells has placed before us. Taking these questions one by one, I should say that, from experiment as well as observation, I should be unable to declare that there is any such thing as a special poison belonging to the puerperal state, or a special poison creating puerperal fever. It is quite true that when puerperal fever is once started, there is a poison formed which will apparently communicate the disease, but there are certain varied forms of this poison, though, perhaps, all these poisons have one common meaning if we could look at them closely; but there are so many

poisons derived from other sources which seem to have the power of producing the disease that this special poison we cannot look upon as we do the poison of small-pox or scarlet fever; neither can we say, in respect to the first question, that there are such local lesions as would lead us in the dissecting room in any case to say, this was a case of puerperal fever, as we should say, this was a case of scarlet fever, or of typhoid, or of typhus fever. I recollect making a *post-mortem* some years ago, in a case of what was called puerperal fever, on the body of a lady of some distinction in this metropolis. There were four eminent members of the profession present, and they were none of them agreed on the pathology. We then began to discuss whether, supposing this case were going before a jury, and we had to give our opinions, we could formulate in any way a series of pathological changes which would indicate the cause of death as due to puerperal fever. Or, to put it in another way, supposing one of us had said, "This is a case of puerperal fever; let us make a *post-mortem* examination;" would any one of us have known precisely what we were going to find as distinguishing that diagnosis? We decided we should not know. I should give a negative answer, therefore, to the question of Mr. Wells, both as regards the special poison and as regards the special characteristic pathology of the disease. In respect to the second question, whether any or all of the forms of puerperal fever may be referred to an attack of infectious continued fever, such as scarlet fever, I should also offer a negative. Under the term puerperal fever, as I have been obliged to recognise it, from being frequently summoned to see cases which are so called, I should say that I have seen four distinct forms of fever which clearly may bear the name. *Firstly*, there is the pure, simple surgical fever, as we may call it, following upon the delivery of the child; I presume there is no such thing (at any rate the exception is very rare) as a case of delivery which is not followed by some slight febrile state, for that state is

necessitated by the changed physiological conditions, by the increased tension of the vessels which must occur from the removal of so much blood in the uterine circulation, and by the irritation that has occurred in the breast previously to the secretion of milk. There is always this simple surgical fever similar to that which occurs after a surgeon has removed a limb—a fever of resistance, as I have named it in a paper that I have written on the subject. I have illustrated this by direct experiments, in which I have shown that when a thermo-electric current is produced, we can cause an increase of heat on the periphery of the current at various points, by cutting off portions of what may be called the thermo-electric circulation. I have seen some cases of so-called puerperal fever which were nothing more than exaggerated forms of this fever, which had become exceedingly severe, but which were instances of true inflammatory fever, and with apparent indications of danger from that source; nay, in one case I have seen a fatal result from the deposition of fibrin in the heart. In that case there was a rise of temperature of 4° F. following the confinement, then a sudden collapse. An exceedingly rich blood, rich in fibrin, had been so modified by that simple rise in temperature, and by the increased friction of the blood passing through the heart, that a deposition of fibrin had taken place in the auricula of the right auricle, that had increased, layers of fibrin had been laid down, layer upon layer, as in aneurism, and ultimately a complete mould of fibrin had been formed; there was an extension of that into the pulmonary artery, a loosening of the whole, and death. Yet not a trace of disease in any part of the patient existed, not in the uterus, not in any organ of the body, except that plugging up of the right side of the heart. We see such charges occurring in simple instances of pneumonia and some other forms of more trifling inflammatory diseases. An attack of erysipelas, with a slight blush in the leg, I have known to terminate in a few hours in death in the same way. *Secondly*, I

have seen another form of puerperal fever (many will recall similar cases) of what may be called a remittent character, with slight symptoms or extreme symptoms of jaundice; twice I have seen this coming on with high fever in the puerperal state. In the practice of the late Mr. Palmer, of Mortlake, this occurred—a pure case of bilious remittent fever, in a neighbourhood where there was no puerperal fever about, ending fatally in a very few hours, with all the conditions of yellow fever. I have seen cases ending fatally with this remittent bilious character pertaining to them in other instances. *Thirdly*, there is the class of cases, such as Mr. Hutchinson referred to, where there is a true introduction into the body of the patient of matter derived probably from the uterine sinuses, and where we get one of the true distinctive forms of septinous poisoning; a form in which death takes place from deficient oxidation, or a form in which there is a separation of fibrin coming on rapidly, and terminating by stopping the circulation of the blood through the right side of the heart. These cases seem to me to be of that class, spoken of by many speakers here, where there has been some local injury, the exposure of cellular tissue, the rupture or exposure of a vein, the formation of a modified secretion, the absorption of that secretion, and death from what Mr. Hutchinson correctly called poisoning immediately from the patient herself. *Lastly*, there is a distinct source of puerperal fever, where a poison appears to be carried into the body from without. And, here comes the singular part of this poisoning, that the poison may belong to any one of the poisonous diseases, or that it may not be, in the strict sense of the word, a poisonous secretion at all, *i. e.* it may be poisonous in the individual sense, but not poisonous as coming from a person who has been poisoned. We have instances of scarlet fever, apparently provoking the puerperal condition in these ways: we have instances of erysipelas; we have examples of poison derived from other puerperal cases, and we have that extra-

ordinary instance, recorded with great faithfulness and honesty by Dr. Huntley, of Jarrow-on-Tyne, where he shows, by a series of conclusive arguments, that a secretion from his own hand was the cause, three times successively, after intervals of several months' practice, of conveying poisonous matter to patients and of so inducing puerperal fever. These are true cases of septinous puerperal fever, where we must assume that the poisonous matter is carried from the hand, or by some other channel, into the uterine surface, where it meets with a favorable reception for absorption, and is conveyed into the body, producing all these phenomena which have been experimentally induced by inoculation. Speaking again on the second question proposed by Mr. Wells, I should repeat that there is no special form of puerperal fever, no one particular type of the disease, but that there are several varied forms of the disease terminating much in the same way, but having distinct characteristics; that some are contagious, some not contagious; that some spring from what we may call a natural condition of the patient, others from an injury of the patient, others from the introduction of morbid material. This question bears on the third question, "If all cases of contagious and infectious disease which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?" I should answer that, barring that natural febrile state which follows upon confinement, as surgical fever follows upon injury, there is no such disease as specific puerperal fever. In this I agree with Dr. Braxton Hicks. As regards the fourth question put forward by Mr. Spencer Wells, the means of preventing the spread of the disease, I have nothing to add to what was said by Dr. Braxton Hicks on the effect of washing out the uterus, and the general measures for removing the source of poison and isolating the patient. I am now brought to the fifth question, relating to the presence of bacteria and allied organic forms in the puerperal state; that, of course, only relates to the pure septinous

condition, the last type of puerperal fevers, to which I have drawn attention. On this matter, as is well known to most of you, I have held for many years—more than twenty-five years—to the purely physical side of the question, and my opinion is in no way changed by the various views which have been more recently announced as to the effect of organic germs and bacteria in the production of these diseases. I am at this moment still more firmly adherent to the physical view from my recent researches, which have shown me the probable mode of action of these septinous poisons, that they all act after the manner of those inorganic bodies, such as black oxide of platinum, manganese, and other bodies of that class, which in infinitely minute quantities have the power of eliminating oxygen from its solutions, and preventing combination of oxygen with the blood. I take it, that the whole of these poisons act in that simple physical manner, and that the presence of organic germs and bacteria is mere matter of coincidence—that where the conditions are favorable for those organic forms to grow and multiply, there they grow and multiply, but that their growth is entirely coincident; that is to say, the further development of the organic form which can be perceived has no more to do with the production of the disease than the presence of the maggot in the decomposing meat has to do with the decomposition of that meat. To put it in a simple way. I remember an observation in my child-life which places the position well before the Society. There is a time in country districts when, as is well known, sheep fall in the field; the sheep may not be really dead, but they are infested by carrion crows, which sometimes injure them severely, picking out their eyes, and doing them a deal of mischief. In my school-days I remember that we had a debate, whether the fall of the sheep was due or not to the presence of these carrion crows. They certainly did a deal of mischief, and some of my school-fellows would go out with stones and sticks to drive them away; but others of us thought that it was

the heaviness of the fleece that caused the sheep to fall; and we would put the sheep on their legs to run about again. It seems to me that in a similar way a pure physical cause is at work in producing the falling down of a woman with disease from the influence of septinous poisoning, and that the living products which are found are coincidences—beautiful specimens of natural history—which, perhaps, are not directly favorable to the progress of cure when once they are developed, but are purely matters of natural history, removed altogether from the true action of septinous poison. I have but a few other remarks to make. My impression is that, in the course of time, we shall arrive at the discovery of certain agents which will immediately stop the action of septinous poisons, by their direct physical effect upon the blood, and their influences in holding oxygen in combination with the blood. I have recently referred, in another society, to the effect of quinine in this respect; but treatment with quinine is a bungling, crude method of dealing with an agent that will act in such proportions as the ten-thousandth or the hundred-thousandth part of a grain, and so produce disturbance within the organism. Dealing with antiseptics, I should say that, if antiseptics, as they are called, that is, bodies which prevent putrefaction, are advanced as a means of curing these particular diseases arising from septinous poisons, their action is not because they are antiseptics (for other agents which are not antiseptics possess a similar property), but for the simple reason that they act on a given principle, and many of them act altogether in accord physically, and I might almost add chemically, in neutralising the specific action of the poisonous agents. I mean that antiseptics do not act by destroying germs or organic forms, but that they act definitely, when they act at all, by interfering with the poisonous action of the septinous material which produces the fatal disease. I predict that in ten years hence, in this Society, we shall know a means of preventing these

diseases from septinous poisons, as clearly as we now know the means of producing them by the introduction into the body of the poisons in the form of inoculated matter, such as the matter of smallpox or of the modified vaccinia.

MAY 5TH, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—73 Fellows and 22 visitors.

Books were presented by Dr. Burggraeve, Dr. William Goodell, Dr. August Martin, Dr. W. L. Richardson, Dr. Alex. D. Sinclair, and Prof. O. Spiegelberg.

Dr. Edwin Hollings was admitted a Fellow of the Society; and Dr. Eugene Rey, of Rome, was declared admitted.

The following gentlemen were elected Fellows:—Thomas Bailey, M.R.C.S. (Godston); Richard B. Cole, M.D. (San Francisco); Robert Cory, M.B.; Walter Henry Drew, M.R.C.P. Edin.; Alfred Thomas Gibbings, M.D.; James Gray, M.D. (Glasgow); and Edwin M. Sheldon (Liverpool).

The adjourned discussion on Puerperal Fever was then resumed.

The PRESIDENT.—It will be in the recollection of the Society that the discussion commenced by Mr. Spencer Wells was adjourned at the last meeting, on the motion of Dr. Barnes, seconded by Dr. Squire; and it will, therefore, be continued this evening. Before the discussion commences, I may state that Dr. Matthews Duncan has

forwarded a letter of explanation in reference to a letter read at the last meeting. He understood that adverse comments had been made on his published letter, and he has, according to request, made further statement of his views, which I am sure will be interesting to the meeting. The letter was as follows:

"*To the Secretary of the Obstetrical Society of London.*

"Edinburgh, May 3rd, 1875.

"My dear Sir,—As I think my former note to you was too short, I beg now to supplement it to a small extent.

"One of the most important points for discussion by the Obstetrical Society at the present juncture is the conduct proper to midwifery practitioners, with a view to avoid disaster to patients from puerperal infection; and I expect much advantage from our great Society taking up this difficult question. At the same time, I feel called upon to deprecate premature decision; for the matter, in both its scientific and practical aspects, is not ripe for more than intelligent, prudent, and fearless discussion. Observing recent trials of our humble and unlicensed female fellow practitioners, I think there have been precipitancy and rashness, believing, as I do, that Mrs. Dymond and Mrs. Marsden have been severely, and probably also unfairly, dealt with. I judge from the meagre reports regarding the cases of these midwives in the weekly medical journals.

"The charge of homicide by infection is, I believe, a new one in the history of the law; and I am further of opinion that, in the present state of science and practice, such a charge cannot be substantiated. I know that Mrs. Marsden is now in prison enduring penalty for homicide by infection, and that this is an example of the charge being sustained; but, while I grieve for this sufferer, I feel sure that the case would have broken down had it been tried in a proper manner.

"The public, and to some extent, also, the professional mind, is not well informed on the subject of puerperal mortality and puerperal infection; and there is much excitement abroad regarding the prevention of disease generally. Under these circumstances, error is easily fallen into, even by authorities, when brought face to face with the terrible facts, probably for the first time.

"I shall say nothing about the amount of puerperal mortality, presuming that many of the members of our Society have read some remarks on the subject in my last year's address at the Norwich meeting of the British Medical Association. But I may remind you that such high and justly respected authorities as Miss Nightingale and Dr. Wm. Farr have promulgated far too favorable views as to the smallness of this amount; and that exaggerated notions of the innocuousness of child-bearing are fostered by the careless talk of many experienced medical men, asserting, as they are often heard to do, that they never lose a case. If it be believed, as these would have it, that there are very few deaths, it is easy to understand the panic excited by the actual facts.

"On another point there seems to be still greater misunderstanding on the part of the public and of the profession, namely, the precautions taken or considered necessary by the profession to avoid communicating disease to patients. For the prevalence of this error, I can cite the evidence of the 'Times' newspaper, whose utterly erroneous statement I now give as I find it in a leading article of that great organ of public opinion. 'It is the invariable practice of medical men,' says the 'Times,' 'if they attend a case of this (puerperal) fever, to hand over the whole of their midwifery practice to other persons, for at least two or three months; and it has been shown by ample experience that this course is absolutely necessary to preserve the lives of their patients.' Anything more inconsistent with truth it would be difficult to find. It is this error, thus widely promulgated, and no doubt entertained, that I wish to point out; but I may

be allowed to add that, were this true, I should never have a chance of being in practice. I should be perennially on the shelf; and so would every one who had a large practice, especially if that practice involved hospital duty and private consultations.

"The difficulties of determining the proper conduct of obstetricians are not to be solved by officious coroners foolishly sending threatening messages to practitioners, nor by judges giving decisions without due consideration; but by discussion in such a Society as ours, and by matured professional opinion. Were the conduct of the coroner in Mrs. Dymond's case, and of the judge in Mrs. Marsden's, to be held as good precedents, there is not one among us who would be secure for a day against the most terrible charges. No surgeon having a case of pyæmia or of erysipelas could go safely to any other patient. No physician having a case of scarlatina could go elsewhere. The lying-in woman, no doubt, requires special care, but such care does not differ in kind from the care of other patients. Homicide by infection may be as easily brought home to the surgeon or physician as to the midwife.

"I regard the recent prosecutions of midwives as injudicious, because I believe it would be difficult to find a physician or surgeon who might not, on the same kind of grounds, be cast into prison. The crime—so called—of Mrs. Marsden is to be found, almost every week lately, confessed by ingenuous practitioners writing in our journals. Moreover, I should have preferred that some *man* should have been first charged with this crime, and not a poor and comparatively defenceless woman; and, as I have said, it will be easy for the authorities to find a criminal man. Where is the physician or surgeon who has not many, many times exposed his patients to some risk of infection with dangerous disease? Is a physician to be found guilty of homicide who exposes his patients to no greater risk than is that of those dearest to him at his home?

"The medical profession, with one voice, proclaims the necessity of extreme care and prudence on the part of medical men, and the utter abnegation of selfish views in their conduct of practice. But there is danger of the public and of many professional men being now led to seek for and expect a degree of safety which can in no ordinary circumstances be reached, and to entertain views as to the conduct of practice which are based on error, and lead astray from the line of expediency and prudence.

"Absolute safety can in no way be attained. As practice is at present conducted by careful practitioners, I believe it is nearly as safe as it can be. Among the necessary precautions in ordinary circumstances, I do not place giving up practice for a time; and I may say, for myself, that in nearly thirty years of obstetric experience in private, in hospital, and in consultation, I have not, as a precaution, given up work for a single day. And I venture to say that it would not astonish me were it demonstrated that as much disease and death come from patients to practitioners and nurses, as go from practitioners and nurses, ordinarily and duly careful, to patients.

"I must draw to a close without having said nearly all I wish to say, and merely name the following grand precautionary measures for obstetric practitioners:

"1. Avoidance of the duties of nurses.

"2. Avoidance of using the hands in *post-mortem* investigation.

"3. Antiseptic cleanliness of the hands and of the dress.

"One case of infection by a practitioner is as heinous as a series of cases. One homicide is as bad as four. The chapter of accidents may bring a series of cases to one practitioner, who is really careful and innocent; and the series may appear to be the result of infection. I have never had a series of cases, not even two near one another, in my private experience; and I believe such an occurrence would drive me away from my prac-

tice, whatever my theoretical views might be. Many practitioners have, in such circumstances, persisted in practice, and do so now, just as Mrs. Marsden did, in my opinion, foolishly, though not criminally, persist. If homicide by infection be a good charge of manslaughter, it is as good in a case of one infection as in a case of four, however appalling the latter may be. There are few practitioners in any department of medicine against whom such a charge might not be set up.

"Allow me to express my regret at forced absence from your meeting, and believe me, dear Sir, yours faithfully,

"J. Matthews Duncan."

Dr. Barnes.—I shall be quite willing to cede my right of priority to any visitor who may be willing to address us. We, who are in the habit of attending here, have spoken so often that I think we must be almost tired of hearing our own voices; and one great advantage of this particular discussion is, I take to be, that it brings into our Society the views entertained by persons who do not usually come here, especially of men practising in different departments of medicine. I have been in the habit of saying that in obstetric practice we may see at least the germs or the illustrations of almost every problem in medicine and surgery. We want, nevertheless, to have the advantage of the additional light thrown upon these problems from all sources. Of late years, owing to the enormous growth of the metropolis, and the enormous number of medical men practising in different departments, the tendency to cut up medicine into sections is attended with this disadvantage, that we cannot, except on rare occasions like this, get the benefit of the varied experience of men bearing upon one particular department. We should be glad to have the advantage of hearing those who practise surgery and medicine; at the same time, I think that these problems of pyæmia and fever may receive some of the most valuable illustrations in the practice

of obstetric medicine. I heard the letter of Dr. Duncan with considerable satisfaction. I think it places the grave and great difficulty which oppresses medical practice before us in a most striking manner. If prosecutions are to go on as they are now doing, I do not know how it will be safe for any one to practise at all. It is becoming a most serious matter. We are governed by the ignorance of the law and the ignorance of the judges. The public are taught improperly. Instruction upon this matter, as Dr. Duncan rightly suggests, ought to come from us; and that is one of the advantages, I hope, of a discussion of this kind. The lawyers and the public must receive, or ought to receive, medical opinions from the medical profession, from experts, those who are skilled and know what they are talking about. It will be dangerous to the public at large, unless this principle is carried out more fully than it is. With regard to the great question of puerperal fever, of course for many years as an old teacher I have had to consider this question; and I have been trying and struggling to put my ideas into a definite form, capable of being understood and taken up by others. I have felt the difficulty expressed by Dr. Leishman at the last meeting; still, after debating the subject in my own mind, I have come to something like an outline of definite ideas, which, of course, I am ready to change to-morrow, or even to-night from what I may hear, as I quite admit the propriety of holding the mind perfectly free and untrammelled, ready to receive any kind of opinion, from whatever source it may come, and especially to study facts and the bearing of those facts. The leading idea, which tends to simplify the investigation of the subject, is one which I took some pains to illustrate some years ago in a series of lectures published in the 'Lancet,' but never finished; that is, that we may at any rate divide the cases of puerperal fever into two great classes, and, having got as far as that, we can proceed a little further with the analysis, and perhaps pursue each series of cases ultimately to their true source. I think we may lay down this

proposition, that we cannot discard the term puerperal fever. Notwithstanding the respect I have for my friend Mr. Hutchinson, whom I look upon as one of the most enlightened authorities of the day, I cannot see my way to discard the term. The meaning attached to it is simply this—fever in a lying-in woman, the general term expressing a number of perfectly different conditions. Having admitted that simply meaning fever in lying-in women, we may proceed to analyse it, and to trace the different varieties. I think we may here divide it in this way. We must all of us see examples of it every day. There is the form of fever in the lying-in woman which is the direct result of infection or contagion produced by some zymotic poison, as scarlet fever (perhaps the most common of all), or erysipelas, or measles, or typhoid. All those things we see and know, and we cannot for a moment dispute them. Well, then, we have a large class of cases, perhaps the most numerous, which we may call heterogenetic, which has arisen outside the patient's body, and been put into her. Then there is another class of cases, which may perhaps admit of more dispute—a class of cases which I should call autogenetic—in which all the conditions of the fever exist or arise in the patient's system, with which infection or contagion from without has nothing to do. That class of cases I am as convinced about as I am about the cases of scarlet fever; they are manifest to every one. But the objection has been raised, and has occurred to most of us, how is it that lying-in women are especially prone to scarlet fever? Like other persons, the great majority of them have had scarlet fever at some previous period of their lives, and may be supposed to be more or less protected, and how is it that their protection all of a sudden breaks down under the trial of childbirth? It is said, again, that scarlet fever can give nothing but scarlet fever, just as an acorn can give nothing but an oak. That may be true to a certain extent. I have seen cases traced to scarlatinal poison in which the usual symptoms

of scarlet fever were absent; no particular sore throat, no swelling of the glands, no rash, and yet the cases have gone on to a fatal issue. Then, if we go a little further, we see what was alluded to by Dr. Richardson, who was, I think, the only speaker at the last meeting who hit upon the right key. If we look at what a lying-in woman is, we there see a peculiar constitution, ready to receive poisons, and ready for those poisons to ferment and go on to a disastrous issue, while in another case the poison has no such effect. Then there is a peculiar condition following labour, where the system has been loaded with matter. There is the involution of the uterus, the discharge of superfluous blood, the milk process coming on—a state which is just treading on the verge of fever; at any moment the slightest excitement or the slightest noxious matter carried into the blood is ready to ferment and set up a fever. It does not matter what the poison is; I think that one observer in his investigation showed that even a case of cancer in a ward was the starting-point of a series of puerperal cases. It may be said that a cancer-germ will produce nothing but cancer. It will produce fever in a lying-in woman; so will scarlet fever always manifest itself in the usual way, which we see in non-pregnant and puerperal women. This peculiar constitution is one which we may recognise in the range of surgery and medicine as well as obstetrics, although in obstetrics we get the most striking illustrations of this, as in many other pathological forms. In surgical practice, for example, we may see sometimes that there are persons, adults especially, who cannot be vaccinated with impunity; we all know cases of that kind; the blood is in a ferment at once. This simple, mild, laudable poison, which we may call vaccine lymph, will set up a ferment, and the patient may die. Sir Benjamin Brodie used to tell us of a man who was killed by the sting of a bee, so great was the irritation set up by the poison, but that argued a peculiar state of the system. We know there are persons who cannot scratch themselves without a fester, all going

wrong in a moment. Well, all that condition exists in an exaggerated degree in the lying-in woman, no matter what the poison is, whether you call it scarlet fever, or measles, or anything else. This is more especially observed in cases where the symptoms are manifested earlier than they are in the other class of cases where the poison arises in the patient's own system. The fever breaks out in twenty-four hours, or in two or three days, whereas in the other cases it comes on later. With regard to scarlet fever, it is enough to set up any mischief in a lying-in woman, and produces all the mischiefs of any other form of poison. We all know that scarlet-fever poison, whatever pathological change it produces in the woman herself, can propagate scarlet fever. Then I would pass to the autogenetic cases. These cases are as distinct in their origin as many cases of infection. For example, you see a woman in the country away from all sources of infection; a little bit of the placenta is obtained, a clot of blood is there, or some change takes place in the uterus, and there is an element of infection; it runs along the veins or the lymphatics, is absorbed in the mucous membrane; then you get the blood tainted, and the slightest matter will set it going; the whole system is in ferment, just as it was from the poison of scarlet fever or anything else; you may call it pyæmia or septicæmia, the result is about the same. These cases come on a little later than those which have zymotic origin, and they can often be arrested by washing out the uterus and bringing away any superfluous matter there. Many women will succumb at once, or rapidly, to a single dose, no matter how small it may be; but others can resist to a certain extent; their excretory organs may be in good working order, and they may throw off a moderate dose, or two moderate doses; they cannot go on surviving repeated doses. If you can wash out the uterus, you may prevent its going on, and stop the disease. That principle has recently come into vogue again. It was practised and taught with success by Harvey, who, if he had not been the greatest

physiologist living, would have been perhaps the foremost obstetrician in the world. This mode of infection is one of very great importance for us to consider. There was a case referred to by Dr. Wilson, a gentleman in the country, who believed that infection was due to the skin. I believe it may be propagated by the breath of a medical attendant or a nurse; we must all be conscious sometimes of taking in poisons by coming in contact with poisonous patients. I have gone away from a craniotomy case with a foul breath, stinking of it; my breath smelling for a day or two. So with other diseases. In a case of dysentery I had shivering, diarrhœa, and foul breath from the odour of dysentery stools two or three days after being in contact with a patient of that kind. So with other diseases. A man may walk about charged with infectious disease, and those who are susceptible with whom he comes in contact may catch it; those who are not may perhaps have a little dose, which they can throw off, the system being in good working order, and there is an end of it; but if a patient be in a lying-in state, with the blood ready to ferment, such a person would be ready to be attacked. There is the secret of the difference. A medical man, who has seen a case of scarlet fever, comes in contact, in the course of the day, with twenty or thirty patients, and it is perhaps only the lying-in woman who takes the disease; not that he is necessarily longer in contact with her, but there is a greater liability on her part. I had a letter this morning from the son of an old member of the Society, a former vice-president, who calls my attention to a work by his late father, Dr. U. West. He gives one example, which is instructive, and I thought you would like to hear it. He had an outbreak of puerperal fever in his practice. He had to deliver a woman, to whom he went straight from a case of erysipelas, and this woman had erysipelas the next day. He went afterwards to another patient, whom he did not examine, and that woman escaped. The next day he went to another case, and the woman died. That is the

history of a great many of the epidemics in the German lying-in hospitals, where they have the faculty, as it seems to me, of manufacturing puerperal fever on a large scale. Those women who are lucky enough (one would consider the most unfortunate) to be confined in the streets on their way to the hospital escape puerperal fever. It is only those who are examined repeatedly that catch it. That has been observed over and over again. If I might be permitted a moment or two longer, I could give you the history of a series of cases occurring in the practice of one midwife in a short time, while all the surrounding neighbourhood was at the time pretty free. I will now only sum up my conclusions in reference to the question submitted by Mr. Spencer Wells—"Did you ever see a case of puerperal fever which is not really a case of scarlatina or rubeola, or erysipelas or traumatic fever caused by the bruising or tearing of parts?" I do not think there is any fever caused by bruising the parts. If there be a little scratch, no matter how small, and the poison is conveyed in that way, that is a different case. That is the way that wounds act so badly, no doubt. We are not to suppose that there is a puerperal fever which runs a definite course like scarlet fever. We get a fever which is fatal, and that is serious enough to be considered as a case of puerperal fever. As to the question, whether there is a real form of puerperal fever which is not scarlatina or any of those conditions specified, I will say that there is, and I have called it excretory puerperal fever. We have albuminuria at the end of pregnancy, and those cases are extremely apt to go into puerperal fever; the blood is peculiarly overcharged, the liver and the kidneys cannot act, excretion is at an end, and then the poison runs in without any further injection of poisonous matter from without. This is pure puerperal fever, which the patient herself can generate. Then, as to how the spread of the disease can be prevented, I can only say, by careful isolation of the patient. The conditions of practice are sometimes incompatible with that.

There was a case that I met with Dr. West; he was on the point of going away, and there was no one to take his place. The patient sent for him, and would have him, and she fell a victim. That happens to every one in the country; he cannot get away when he likes. With regard to the question of bacteria, as I know nothing about it, I had better say nothing; but I may reserve my doubt with Dr. Richardson, and wait till the bacteria doctrine is proved by those who understand it. With regard to the value of antiseptics, in order to keep hospitals free, I think that to keep hospitals free from puerperal fever is an extremely difficult matter,—more difficult than it is in a surgical hospital to keep it free from pyæmia. You cannot keep a series of patients in a hospital isolated, in the proper sense of the word. You have the same nurses going in to them. You have a variety of poisons acting upon one or two patients, and the consequences may be radiated to others. There is only one secret for safety, and that is to have the woman confined at her own home, where she can have her own nurse, who has not been anywhere in the way of infection, and her own medical man, who is free from infection. Then the chances are that she will go on favorably and happily. Without that there is no security. A lying-in hospital is not now, by any means, so serious a matter as it used to be; still, it is always like sitting on a volcano which will explode at any moment.

Dr. Squire.—I am sorry that the limitation of time has made the valuable observations of Dr. Barnes a little hurried. I must say that, agreeing with him so much, I was a little surprised to hear him support a statement of Dr. Richardson's as to the very febrile condition of lying-in women, an idea which I thought was derived from the depths of his inner consciousness, and not as the result of bedside experience. In the course of a very large inquiry I have failed to find that febrile disturbance in the majority of women in well-to-do circumstances.

This important subject was ably brought before us for discussion under two divisions. It is to the queries under the first of these divisions that I wish to reply; and it is to the second question that I intend to confine myself more particularly. I presume that there *is* a fever after childbirth caused by a morbid poison communicable by infection; and, while admitting a large class of infections associated with puerperal fever, I hope to show that the whole class of acute specific diseases may be set aside. It may be one extreme of error to deny the existence of puerperal fever; it is certainly the other extreme to call every *post-partum* illness by that name. Almost any febrile attack in the puerperal state will cause suppression of the lochia, arrest of the lacteal secretion, and other common symptoms, so that we have to separate many febrile ailments, as well as what may happen from retained placenta or other causes of metritis and phlebitis, from the disease under consideration. Our first duty in meeting with such cases is not to call them by the name of puerperal fever, but to diagnose their nature. Many of them we can soon relieve, and in all be sure of carrying no infection to others; while no case of puerperal fever is without this risk. Question two, deals with the relations of puerperal fever: first, to the specific infectious diseases; and second, to the traumatic fevers and septicæmia. Clear as was the introductory statement, I noticed that, in repeating the question, Mr. Wells transposed erysipelas from the second category to the first. I do not mean to deny it a place there; but, for the present purpose, prefer to keep to the terms of the original proposition. I am then able to answer the second part in the affirmative, and to give to the first part a denial as precise and distinct as possible. I go beyond this, and say that no form of puerperal fever is to be referred to attacks of the specific infective fevers. The 'Transactions' of our Society, abounding in excellent materials for the subject in debate, furnish more commentaries against the view I take than in its support. In the very carefully prepared

paper in vol. iii I find it stated from the chair that "any of the agents which produce zymotic maladies might cause puerperal fever;" and, again, in vol. x, the President, among remarks of high value, says "it can be caused by the contagion of typhus, measles, and scarlet fever." At the last meeting this opinion seemed general. This view has led some to look upon this disease as merely the puerperal form of scarlet fever, or to consider it typhus. This view I controvert, and assert that not only is puerperal fever not typhus, typhoid, smallpox, measles, diphtheria, or even scarlatina, but that these diseases are little modified by the puerperal state and retain their distinctive characters so as to be recognisable; they ought to be diagnosed under this, as under other conditions, and called by their own names, and not by that of puerperal fever. Moreover, though pregnancy and puerperal fever may prejudice the prognosis in such complications, and abortion happens in some of them, yet the puerperal accidents are not always so grave as is supposed. I will first take typhus. Of 1000 typhus patients admitted to the London Fever Hospital sixteen were far advanced in pregnancy; they miscarried, and the majority made good recoveries. This is very different from the results of abortion in typhoid, where, however, the fatal result is owing to the serious importance of this accident to the fever, and not to the development of puerperal mischief. As it is quite possible for puerperal fever or purulent infection to occur after abortion, even in the earlier months, we see nothing in typhus to cause puerperal fever. Yet I have a good authority against me. Sir Henry Marsh, in the fourth volume of the 'Dublin Hospital Reports' for 1827, says—"The true character of epidemic puerperal fever seems to be typhus." This is founded on the experience of Dr. Johnson, Professor of Midwifery to the College of Surgeons of Ireland, who states that the wardmaids of the Dublin Lying-in Hospital caught typhus from patients received there. Typhus was prevalent at Dublin, and this shows how typhus may find its

way into a lying-in hospital. Twenty years ago there was typhus in Leeds. A friend of mine attending to midwifery in the Leeds School of Medicine met with a case of typhus at childbirth; the fever was well marked; it commenced before delivery, and the characteristic mulberry rash occurred. It was not communicated to any other of numerous patients consigned to his care, though some cases of puerperal fever were met with in the practice of other students.

It is not contended that smallpox is modified by the puerperal state. Vaccination saves us from meeting with it in the severe form; but I have one instance where it was easily recognised, and did not produce puerperal fever. When I saw the patient she was in severe pain; she had gone her full time; she looked ill, and the temperature was 104°. That being utterly different from anything I had found in an ordinary case of labour, and being what occurs in the ingress of few ailments besides smallpox, I made inquiries, and found that a young man, sickening with smallpox, had been removed from a house in which she was. The next day labour pains came on, and in the night she was delivered. The spots of smallpox came out on the following day. The child was removed, and vaccinated at the same time. The woman made a good recovery, without any puerperal symptoms whatever. Now, if the poison of smallpox can produce puerperal fever, that was a case in which it ought to have occurred.

In measles it is rare to find parturient women unprotected by a previous attack; but when so protected, I have seen a mother nurse her children through measles without the least injury up to the time of delivery. Lately I saw such an instance, where the poison must have been concentrated in the room for the baby had a rash a fortnight after birth, the mother keeping quite well. Therefore, the statement that measles will convey contagion to parturient women seems doubtful. I will quote one instance from the 'Sydney Papers' of measles in a parturient woman. Lady Sidney was sickening for measles

when, on third day, with severe cough and full rash, "she is brought to bed of a goodly fat son; the child was also full of the measles, mostly in the face, yet it sucked the nurse as well as any child could." Both did well.

With respect to scarlet fever, no one has a more wholesome dread of it than I. Every means of isolation should be used to keep it out of families. Once it happened to me to see scarlet fever in the family of a physician who devotes his time to the study of how to prevent disease, when the mother had just been delivered; she took the disease. Not only were there no ill-symptoms, but not a trace of puerperal disease. The mother had never had scarlet fever before, yet it is quite certain her illness was scarlet fever now, for the infant took the disease; all did well. Dr. Braxton Hicks, in his admirable paper in vol. xii of our 'Transactions,' gives the most valuable support to my argument by his cases, though we differ in the conclusions to be drawn from them. Of his 89 cases but one is entered as truly puerperal fever; and of the 39 cases of scarlet fever there was no difficulty in the diagnosis, 20 of them having well-marked rash. The great point demonstrated by these cases is the special liability of puerperal women to scarlet fever; infection is resisted up to the time of delivery, then they succumb in three or four days. Diphtheria at the end of pregnancy lately caused me much anxiety; to my surprise, soon after delivery the exudation, instead of extending, cleared off, and an obstinate ulceration soon healed. On turning to our 'Transactions' for a case of post-partum diphtheria I find it headed "Mild Puerperal Fever."

When we come to erysipelas the case is very different. Dr. Rigby noticed that the children born during an epidemic of puerperal fever had erysipelas. I do not mean to say that puerperal fever is erysipelas, but this and the King's College outbreak show its close relation to erysipelas, to hospitalism, to purulent infection, and to suppurating wounds. The development of Bacteria is greatly favoured by a high body-temperature, which nervous shock or disease readily excites in the puerperal

state; when this coincides with the introduction of septic particles from without, the worst consequences may result. I believe we can connect puerperal fever closely with that class of infectious diseases which the investigations of Billroth and Lister have not only enabled us to understand, but to control. I believe there is danger in those dissecting or dressing surgical wounds attending midwifery cases. I have seen a dissector suffer from peritonitis after operating on a bad part; what was induced in him he might have been the means of conveying to others.

Dr. Braxton Hicks.—A remark fell from Dr. Squire with respect to the rash accompanying local cases of scarlatina. I think he will find a large number of such cases. Certainly my observations have gone to show that there is no rash, and that there are none of the permanent symptoms of scarlatina where scarlatina has been unquestionably mixed up with the case.

Dr. Brunton.—Mr. President and Gentlemen, I feel called upon to rise and make a few remarks upon the subject now under discussion, because, as a general practitioner, I think that this subject of the power of carrying infection from one patient to another is of vast importance to all who are in the habit of attending all sorts of infectious diseases, and at the same time attending midwifery cases. If the poison of contagious diseases be so productive of puerperal fever as it has been pronounced to be by many of the speakers in this assembly, I cannot help saying that, in my own practice, I have certainly failed to find it such. I come forward as a practical man. Theory is all very well; but, when we come into general practice, one must look at what is seen in every day's work. The first midwifery case I ever attended was in the smallpox ward of the Royal Infirmary, Glasgow. The patient was in the full bloom of smallpox, and aborted about the seventh month. She recovered without any puerperal symptom of any

kind. Then, as Dr. Leishman has said, the students at the University of Glasgow, during the time they are dissecting, are in the habit of attending midwifery cases. I did the same, and I know some gentlemen in the room, who were fellow-students of mine, who also did the same, and, during the whole time of our practice, we had no puerperal fever at all. Then I came to practise in London; on more than one occasion I attended lying-in women who had children lying ill with the scarlet fever in the same room; each made a good recovery, with not the slightest feverish symptom whatever. Then, again, general practitioners are in the habit of making *post-mortem* examinations. They meet with cases of sudden death, and the coroner calls upon them to perform *post-mortem* examinations. I have done it over and over again. I have gone on with my obstetrical practice all the time, and I have not had, in the whole course of my practice, one single case of puerperal fever. I saw one case of puerperal scarlet fever many years ago, where the patient went through the whole course of the disease, with very putrid discharges, as offensive as could well be, and yet the patient recovered. The skin peeled off; she had the usual affection of the kidneys and so on, and yet did well. My evidence in this discussion is decidedly negative. It appears to me that, if scarlet fever, typhus, measles, and smallpox are to be reckoned such very strong producers of puerperal fever, I ought to have had a great deal of it in my practice; but that has not been the case. I will read a note that I received the other day from Dr. Caskie. "A few months ago I was sent to see Mr. P—. I found him covered with the scarlet-fever rash, and by his side his wife in bed, three days previously confined. Her labour had been natural. The husband was taken away, and she made a good recovery. Her own children escaped, but the children of the landlady in the house took the fever, and two of them died." Now, if scarlet fever in that case were so deadly, Mrs. P— ought to have taken it and passed through the

usual course, or died. I cannot help referring to the remark made by Dr. Richardson and Dr. Barnes about the peculiar physiological condition of lying-in women. It is said that at that time their blood is in a peculiar condition. Certainly that is the case; but I look upon it from another point of view. While there is an excess of fibrin and a diminution of blood-corpuscles, a diminution of solids, and an excess of albumen, I look upon this as Nature's preparation for the casting-off of the ovum. I look upon the excess of fibrin as Nature's safeguard against hæmorrhage. I also look upon the excess of fibrin as one of the means for the easy production of the lacteal secretion to follow. Mention was also made of the feverish condition that is said to follow the immediate birth of the child. Since the last meeting I have had the opportunity of speaking to Dr. Richardson, and I have made observations on this point, and in only one case have I been able to find any increase of temperature worth speaking of, except in one case which was not attended by myself, and in which the lady desired not to suckle her child. There, while the pulse remained at 72, the temperature was as high as 104°; the breasts were very hard, showing some sort of rapid tissue-change at all events. What appears to me to be the nature of puerperal fever, as far as I can theoretically make out, is this, that it is simply autogenetic; that it begins in the uterus of the female who has very lately had hæmorrhage, or is weakly, and there is an imperfect contraction of the uterus; a clot remains, and that clot decomposes, and then you have pyæmic poison; then I do not deny that the poison coming off the excreta from the woman in this pyæmic condition may be infectious to others. I regard such a woman as being in a similar condition to one who has had a thigh amputated. We know that if pyæmia occurs after such an operation the dressers are not allowed to dress the wounds of any other patients. No doubt, as Dr. Barnes has said, during this process of pyæmia the poison may be absorbed and then excreted by the lungs of the individual who is in attend-

ance, and be carried to another patient, and there set up a similar form of disease. While Dr. Barnes was speaking I came to the conclusion, from what he said, that, if the contagious diseases are so productive of puerperal fever, and if the medical man be a focus of infection by exhaling the poison, the judges at the present day should look upon a man as criminal if he attend obstetric cases while he is attending a fever case. I must say that my experience is exactly the opposite of that.

Dr. Huntley (Jarrow).—Mr. President and Gentlemen, in responding to your invitation to strangers, I confess I feel some diffidence, in part arising from the fact that Dame Nature has not endowed me with that glibness of tongue which stands its possessor in such service on occasions of this kind, but chiefly arising from the fact that I am addressing many of the most distinguished members of our profession, and it would ill become me, a stranger and of no repute amongst you, to attempt to dogmatise; but as my experience has been somewhat peculiar, of a nature that does not occur to many medical men (perhaps not a single member here present has had a similar experience), I may be allowed to give rather freer expressions to my opinions than otherwise I might do. I will briefly describe to you the experience to which I refer. About ten years ago, in December, 1864, I had an outbreak of puerperal fever in my practice. I was then a very young man, and the anguish of mind which it caused me was something which I shall never forget. In the course of two months I attended fourteen cases of labour, five of which died. All the cases that I attended were not affected by the disease; at intervals two or three escaped; however, at the end of two months I think two patients died. Then I was obliged to cease practice, having come to the conclusion that the disease was in some measure connected with myself; for although there were four other practitioners in our district, not one had a

case of the kind. I was forced to the conclusion that coincidence could not account for such a continued sequence of cases, and that I was the cause of propagating the disease. But how could I propagate it? That was a matter that exercised my mind for many a day. We had smallpox very prevalent at the time in our midst, but no other infectious disease. I was then attending a case of severe burns with great suppuration, but that I only dressed for a few days, and I think it is very unlikely that the infection could be carried from such a source for two months continuously. If it were so, I think such instances would be very frequent, and I could not but consider it absurd in the extreme to think that for weeks, considering all the precautions which I took, frequent ablution, and change of clothes, I still kept the disease. It seemed to me then, and I have no reason to change my opinion now, that the disease was somehow or other reproduced; and the only source from which I thought it could be reproduced was from some poison generated in mal-assimilation, or in some defect in the secretory or excretory system. Arguing from analogy, I see no reason to doubt that many infectious diseases are propagated in this way, and not from clothing. Suppose a nurse is attending a case of scarlet fever for weeks, does she not have the germs of scarlatinal poison entering her system and being exhaled from it, and can we suppose that the disease may not be communicated? There is no proof at all that the clothes communicate the disease always. Perhaps there are instances in which they do. In the case of washerwomen it has been communicated in that way; but I think it is just as reasonable to suppose that vitalised material will be allied with the poison as unvitalised. But it was not chiefly to relate this experience that I came here to-night. I had an impression, from reading the report of the last meeting, that opinions were expressed which were very far from correct. The opinions I refer to are those chiefly discussed by the two previous speakers. I think it was Dr. Braxton Hicks

who asserted that scarlatina was a very prolific source of puerperal fever. But in tracing infection in such a way you are very likely to drift into error, for if you approach the subject with any fixed opinion of your own, the wish is apt to become father to the thought, and you are brought to a conclusion not in accordance with facts. I have never in my own practice been able to associate scarlatinal poison distinctly with puerperal fever. On the 12th November last I was attending two severe cases of scarlet fever occurring in children. The mother of these children took scarlet fever, and the symptoms in her case differed not in the least iota from those of the children. I can call one or two other instances to mind where such has been the case. So with smallpox. I have seen two or three cases of smallpox in parturient women, but they have always manifested the characteristic eruption, and there could be no doubt about stamping them as smallpox. In the case of scarlet fever to which I referred I may say that the woman had no symptoms of inflammation of the uterus, or anything at all to distinguish it. I think what we should really do is not to argue from a particular case to a general conclusion, but to take general facts and deduce particulars from them. I regret to say that the returns of the Registrar-General are not useful in this respect in assisting us to arrive at a satisfactory basis. The returns of puerperal fever, I think, are annual, and extend to counties. If these returns were given quarterly in particular districts, we should then be able to see whether scarlet fever occurred synchronously with the puerperal fever, and then we should have a safe guide.

Dr. Brown.—About six weeks ago I was engaged in attending a patient in her confinement. At that time a child in the house (her own house) was taken ill with a rather severe attack of scarlet fever. She was under great apprehension at the time of her confinement, expecting daily that she would suffer from scarlet fever.

About a fortnight after the rash came out on the child she was confined, and did perfectly well, without a bad symptom. The infant had no sign of scarlet fever. About ten days after that another child in the house had scarlet fever badly. I attended another similar case about two months ago. The patient had never had scarlet fever herself—she was quite positive of it. In another case I attended a woman while her own child was ill with the scarlet fever in the house. In some days she was laid up for her confinement, and she got up without a bad symptom. She had never had scarlet fever. I may mention that I attend on an average three or four midwifery cases a week, and I have never had a case of scarlet fever in which I could say that I had conveyed the disease to the patient.

Dr. SWAYNE (Clifton).—I think the letter read at the beginning of this meeting is calculated to do great good, especially as regards the unfounded statement that appeared in the 'Times' that medical men should retire from practice for two or three months. The statement has in some places created a panic that is becoming quite a nuisance. I will give an instance that occurred to myself. About the 16th of March last I consented reluctantly to see a bad case of puerperal fever. I listened to the solicitations of the husband, but I only saw the patient once. A gentleman whose wife I had engaged to attend at the beginning of this month happened to hear of it, and wrote to me requesting me to release her from the engagement, which, of course, I did. She is not yet confined. As to the mode in which the infection of puerperal fever is conveyed, I think that it is generally by the person of the accoucheur more than by the clothes or any other way. Some men are peculiarly unfortunate in this respect. It is often observed that all the puerperal cases in a district are limited to a few practitioners. It is not only that they have the run of them at a particular time, but even after

a long interval it is the same men who get them. Some men, I believe, have the power of absorbing and exhaling these poisons to a much greater extent than others; and from remarks I have made in my own experience I am inclined to think that the poison is much more likely to be given off by the skin than by the breath of the practitioner. I have come to the conclusion that men who have moist, perspirable skins, especially moist hands, are much more likely to exhale it than those whose hands are generally dry and cool, especially if they have to make frequent examinations during labour. With regard to the precautions to be taken to prevent the spread of puerperal fever, I cannot think that it is necessary for a medical man to seclude himself for more than a week, at the outside, from midwifery practice. If he be unfortunate enough to have a bad case, probably by that time the poison will have passed out of the system. He should also take the precaution of not wearing the same clothes. I am not aware of having conveyed anything of a puerperal kind to a patient except once; that was in a case of scarlet fever. As a general rule, I now refuse to go to a case of scarlet fever, unless, of course, it should occur to a lying-in woman. I refuse to see children with scarlet fever, on account of the danger of carrying it. Some years ago, before I adopted this rule, I was attending a child with scarlet fever, and I confined a lady at the same time. About a week afterwards the rash came out with sore throat; she did perfectly well, and did not show any symptoms of puerperal fever. But the worst case of scarlet fever I ever saw was in a lady who, about a week before her confinement, called at a house where the fever was; the children were ill, and she was afraid to go in, but most imprudently the mother came out directly from the sick room to report how they were going on, and she put her head into the carriage to talk to this lady. In that case the scarlet fever came on *pari passu* with the labour. I attended her about ten o'clock at night. I observed that the face became very red, and at the latter part of the

labour the speech was rather muffled. The redness of the face did not excite particular attention, as it is so usual during the second stage of labour. On the next day I found she had been delirious; she was covered with a thick rash, and had a sore throat. She died in two days. Immediately after seeing a case of puerperal fever I go home, and before going to bed take a warm bath and wash myself with carbolic soap; and on the next day I take a Turkish bath, which, I think, is an excellent way of eliminating the poison and cleansing the skin. I need hardly say that, with every accoucheur, personal cleanliness is of the greatest possible importance. We know that cleanliness is next to godliness; we cannot be too particular about it, especially after seeing cases of this kind; and we should be very careful not to wear the same clothes. With these precautions, in a few days, there will be little or no danger of conveying the poison from one patient to another.

Dr. Graily Hewitt.—The subject which is now before the Society is one which has on many occasions interested me very deeply. I have seen a great deal of this disease, in public and in private, and it has been my duty not only to learn what the disease is, but to endeavour to combat it, and to teach others in the best manner I could how to deal with it. I think this discussion is extremely opportune, and that it is likely to lead to very good results. Our thanks are due to the President and to Mr. Spencer Wells, who have endeavoured to bring the matter before this great Society. I will not dwell further on preliminaries, but go at once *in medias res*. The opinion which I have formed with respect to puerperal fever is, that it is essentially a form of blood-poisoning. Seven or eight years ago I read a paper before this Society, in which I detailed the experience which had occurred to me in the British Lying-in Hospital, with which I had been connected for some time. I brought forward a considerable number of cases, and in giving my conclusion in

reference to the nature of the malady I expressed myself in these words :—" It is impossible to escape the conclusion that puerperal fever consists in nothing more or less than an injection into the general circulating fluid of a poisonous material of animal origin—that is, a form of pyæmia for the production of which the minutest portion of the morbific agent may prove sufficient." I was very early impressed with a lecture, published by the late lamented Sir James Simpson, of Edinburgh, I think, about 1859, in which he strongly advocated the doctrine that puerperal fever is a form of pyæmia ; and what I had seen at that time, and what I have seen since, has led me to endorse that view of the matter in the strongest manner possible. I may say that I entirely disbelieve in the existence of a form of fever which is sufficiently definite and precise to receive a distinctive name, in the same sense as we speak of typhus fever, or typhoid fever, or measles, or scarlet fever, or smallpox, each of which has a definite and well-determined course. I can see nothing in puerperal fever which at all resembles this. I think all the clinical evidence producible on the subject is entirely opposed to that view, and, therefore, I have no hesitation in answering that part of the question put by Mr. Spencer Wells in the way I have done. I think it would be convenient to divide the cases of puerperal fever, or puerperal pyæmia, into two classes. There are—*a*, cases in which there is very distinct evidence of the introduction into the system from without of a morbid animal poison ; there are, in the second place, *b*, cases which do not resemble these, in which the evidence is wanting of the introduction from without of such a morbific poison ; and I will endeavour to make my remarks under those two heads. In the first place, in reference to the cases that occur from the introduction of poison from without, after what has been said on this occasion and on former occasions, there can be no question that it has over and over again happened that the disease called puerperal fever has been produced by inoculation (the word seems to me to be

suitable), in those unfortunate cases where the medical man himself conveys the disease, and in those cases where the midwives are equally efficacious in communicating it. It is to that class of cases that I would first call attention. It is my impression from all that I have seen that the manner in which the communication takes place is, in a large proportion of cases, by means of the hand; and I believe that the spaces beneath the nails and under the skin which covers the nails are exceedingly liable to harbour these destructive animal products. I do not say that they are limited to those parts; the skin of the fingers may also be their habitat. We know what sometimes takes place after a *post-mortem* examination, when it is exceedingly difficult to get rid of the smell of the corpse from the hands, cleanse them in whatever way you will. I am speaking now more particularly of what happened some years ago, before carbolic acid and other disinfectants were much known or used; it is not so difficult now as it was; at all events, I believe that the nails and the neighbourhood of the nails are the parts more particularly liable to harbour these infecting materials. It seems to me rather unlikely that the clothes carry infection so readily. Of course, in cases where a person allows the cuffs of his coat to be dipped in putrescent material, and then makes an examination of a patient in childbirth, something may happen after that. On the whole, I think the hand is particularly efficacious in this matter. I share the opinion that some previous speakers have expressed, that very great attention should be bestowed on cleansing the hands in all cases where they are to be used in midwifery practice. A good deal has been said of late years in reference to the *armamentaria* that medical men should be provided with, and I think a useful addition would be a pot of carbolic ointment and a nail-brush. In the next place, I would remark that it has seemed to me that any animal poison introduced from without may produce what we term puerperal pyæmia, the same as may be produced by a student who is handling surgical wounds in the

hospital and attending midwifery cases. I have seen instances of this in the case of a medical practitioner's going from one puerperal case to another. I see no difference in the attack produced in these two different ways; there is nothing whatever different in regard to the clinical features of them. Another mode in which the poison is introduced from without is due to the carelessness in washing the patient, or an improper method of washing. I think water never ought to be used for cleansing the perinæum in the case of a recently delivered woman. I think it is far preferable to use a dry, clean rag, or dry cotton-wool. This plan secures greater immunity from the passage of *débris* from without. Then, there is another method by which poison may be introduced from without, that is, in case of laceration of the perinæum. I think this mode of introduction is not very uncommon. I recollect being called into consultation to see a lady who was suffering from puerperal pyæmia. Very great care had been taken with her to keep her exceedingly quiet, and she had not been allowed to be moved from her position (lying on the back) for some days after labour. The perinæum had been slightly torn. Now, in this case the discharges were putrescent. It seems hardly possible to escape the conclusion that in such cases as this there is a pool of putrescent material in the vagina constantly in contact with the abraded perinæal surfaces; is it to be wondered at if some of this is absorbed? I think it is bad practice in all cases to maintain the patient too persistently in this position on the back. I think there is an advantage in turning her from side to side occasionally, in order to allow the escape of putrescent *débris* from the vagina—not that it will produce pyæmia in all cases, but if there be a laceration of the perinæum it is more likely to do so. So far for the cases which come under the first category. I will now proceed to discuss those which are more difficult to define and particularise—those in which (*b*) the affection does not apparently depend upon anything introduced from without.

This class corresponds to the cases denominated by Dr. Barnes autogenetic. In the first place, I would remark that it is exceedingly easy to produce pyæmia in the non-puerperal woman by handling the uterus in a certain way. I have seen acute pyæmia produced by the action of a sponge-tent at the os uteri, left in too long, and producing violent inflammatory symptoms, and death in a few days. It is well known that this event may happen. The uterus is so constructed that it is exceedingly easy for pyæmia to occur in it and in its tissues; the walls are very thick, the vessels are very large, and they communicate very freely (I am now speaking of the uterus in the non-puerperal state), and a slight wound may at any moment set up pyæmia. I have always been unable to see any precise difference between an attack of pyæmia in a puerperal state and in a non-puerperal state; the symptoms are precisely identical, they only differ in degree. I do not wish to give my remarks the semblance of a lecture, but I thought it might be interesting to bring a specimen representing the healthy uterus immediately after delivery, in order to show the condition of the organ at that moment. I have also here a plate exhibiting the condition of the uterus after delivery. Doubtless most teachers of midwifery are familiar with it. It will serve as a peg on which to hang the remarks to which I shall call your attention in the next place. In the uterus, after delivery, we have an organ precisely analogous to a sponge. It has large interstices, large spaces freely communicating, and, moreover, these spaces open into the uterus through the vessels which have communicated with the placenta, and these openings are filled up by certain clots. The natural event after labour, of course, is that the uterus becomes contracted. A progressive diminution occurs in its size. This is the safeguard, and this is the important vital phenomenon which belongs to the after-period of childbirth. Now, it is my belief and conviction, from all I have seen, that if we wish to obtain a clue to the manner in which puerperal pyæmia is produced, we

must regard this process very attentively, and notice particularly the deviations from the normal physiological process which are liable to occur. It is some years now since my attention was first directed to the fact, but it is a fact which I have been able to confirm by repeated observation since, and which I deem to be of the greatest importance in reference to this matter, that concurrently with the commencement of the attack of puerperal pyæmia the uterus is found to be enlarged, in other words, in a state in which its involution is absolutely retarded. I have never seen a case of puerperal pyæmia in which this condition of the uterus was absent, and absent at the very commencement of the malady. It seems to me, if this be a fact, that it is an exceedingly important element in the explanation of cases of puerperal pyæmia, of whatever kind they may be. It has appeared to me, in endeavouring to carry out explanations of these cases, that the thing which fails is the contraction of the uterus. That is where the breakdown originally occurs. The woman has a large bleeding, for instance, at the time of childbirth. The want of contraction in the uterus acts in precisely the same way as if all the locks of a house were taken off and ingress allowed to any burglar who wished to enter the premises. This is precisely what happens, in fact, after a labour. The contractile power of the uterus fails to a certain extent. The expulsion of the *débris* ceases, and there occurs a suction-action in the uterus, by which the *débris* is taken up into the circulation. I explain in this way those cases in which these scarlet-fever poisons and other fever poisons apparently produce disease. They destroy the vitality of the patient to a certain extent; they take away the safeguard; they abolish the contraction of the uterus, and they produce a paralysis of the organ for the time being; and when they have once produced this paralysis the rest of the explanation is sufficiently simple. The pyæmic process immediately passes into the uterine sinuses, and it is a question whether the patient will sur-

vive, or whether the injuries in this manner will end in speedy death.

Mr. Callender.—I came here to listen and to learn, but, as you have called upon me, I feel bound to offer some remarks on the subject under consideration; and I do so the more willingly because I quite agree with what fell from Dr. Barnes with reference to the fact, that although we may be engaged at various points of what I may call the professional circle, yet we are all attracted to one common centre, and at that centre I read the words, "prevention and cure of disease." I must be allowed to draw upon my experience as a surgeon in referring to this matter, and I feel that you will be the more indulgent to me when I recall the fact already referred to by several speakers, that there seem to be many points of resemblance between affections such as erysipelas and septicæmia, which are constantly coming under our notice, and the disease, puerperal fever, which is at present under discussion, and which has been held by some speakers to be, in many instances, due in its origin to septic causes. I think that, amongst others, one speaker said that at least three fourths of the cases were traceable to such an origin. With regard to my experience of septicæmia, I prefer to limit myself to this term; for if I ventured to speak of other affections which I have heard mentioned, and which are closely allied to this disease, such as those in which the veins are implicated, I fear, after the remarks that have been made by Mr. Jonathan Hutchinson, I should be regarded as almost a heretic if I ventured to propagate the views I hold as to the part which the veins take in these affections. I confine myself to septicæmic affections; and I say, so far as I can judge from practical experience, that we must consider them with reference to two distinctive sets of signs, those which may be spoken of as speculative, and those which may be regarded as facts. With regard to the speculative matters, I think it is purely a matter of speculation (although there seems to be a common consensus that

such is the case) that these affections are due to the influence of some septic matter or poison. Beyond this it seems to me quite a matter of speculation as to whence the poison is supposed to come. There are some who hold the idea that the poison is to be found in germs which come from without the patient; there are others who seem to hold the view that the septic poison is generated in the woman herself, as the result of the product of decomposition; and there are yet others who say that the septic material is due directly to the products of inflammation. All these, I hold, are mere matters of speculation; what I should like to put before the Society is the question, what series of facts have we to bring to bear upon these various speculations which have been offered to us. Speaking entirely from a practical point of view (I must be allowed to speak from my own experience) there are certain points which have forced themselves on my attention—certain negative facts, such as these. I hold that, in the treatment of patients in a hospital ward, the first point we have to attend to is, that there must be no foulness about the wounds; the wounds must be absolutely clean; directly we see anything unpleasant about them we must take care to remove it. The point is that no foulness should be capable of being recognised; the wounds should be most carefully kept from any contamination from other wounds both by the nurses and dressers; and in regard to sponges and such-like materials, all possibility of contagion from this source should be avoided. This leads me to remark that, in the treatment of wounds, a patient will be tolerant enough of decomposition which may be set up in a wound on his own body, but will be intolerant of poison conveyed to him or her from any other source. That is another point on which we must be extremely careful. The possibility of a wound being contaminated from another wound is a fact that is constantly forced upon our notice. Then the patients should as far as possible be isolated. It is easy enough

to ensure this in a surgical ward, where a case in which there is an open wound may be surrounded by cases not suffering from wounds, so that they can be, for all practical purposes, isolated. No discharge should be allowed to accumulate in a wound; not merely the discharge resulting as a consequence of ordinary inflammatory process, but the discharge thrown out of the wound during the first few hours after it has been inflicted. I know nothing more irritating or acrid than the fluid in a wound immediately after an operation, if it be allowed to accumulate. Then, again (and I think this is another point of great negative moment), there should be no movement of the part; the wound should be kept absolutely at rest, allowed to heal from the beginning, and not permitted, by jars and jolts, to reopen, so as to create new wounds, through the cracks and fissures of which putrid or other foreign matters may enter. By keeping these facts in view we have learned how to banish, for all practical purposes, such affections as those commonly called pyæmic (though I prefer the term septicæmic), from surgical wards, save when we commit errors, and troubles arise in consequence. No such thing as pyæmia exists in the hospital wards under my immediate care. By the experience we get from these facts we have some confirmation of the worst of the suspicions that I spoke of, namely, the suspicion that these cases of septicæmia are in some way connected with the presence of a poison. I think, also, we have some light thrown upon what is patent to us all—that which has been ably spoken of this evening by Dr. Graily Hewitt, the sort of common-sense rule that should guide us with regard to the treatment of women in a puerperal state, such as the maintenance of absolute cleanliness and absolute quietude. We do not, however, from these facts, get much light thrown upon what I further spoke of as a matter of suspicion regarding the cause of the disease. I do not at all see my way clear as to the exact source of the poison; it may be in the air; it may

be brought to the patient through her attendant; it may possibly be in the festering of a wound, or be present in the direct products of inflammation, but I do not see my way to the absolute source. All one can see is, from the collection of facts surrounding my cases and extending over a considerable time, that septicæmia is a disease which in surgical practice, may be absolutely prevented. Here I should like to answer one of Mr. Spencer Wells' questions, whether the disease can possibly be produced. I entertain myself no doubt that pyæmia and septicæmia can be produced. If, after I had performed an operation upon a patient in otherwise good health, I left that patient unprotected by all the many means that one is bound to adopt to ensure, as a matter of certainty, that a case shall be conducted towards a successful termination, it would be a mere matter of chance whether that patient did or did not suffer from pyæmia. I say a matter of chance, because people differ very much among themselves; a patient may be strong and robust, and may resist any poison brought into contact with the system, or the patient may be feeble, and may readily succumb to the slightest dose of poison. With reference to another question, I would say, before I sit down, that I hold that antiseptics are of very great use to surgeons. I think they are of the greatest possible use in surgical practice; and I take this opportunity, as it is a public one, of saying that, although I may have seemed to differ very much from Mr. Lister with regard to carrying out the principles of treatment, I am entirely at one with him as to the value of antiseptics, and I fully acknowledge the great benefit he has conferred upon us by his advocacy of the antiseptic remedies. What I wish to show is that the treatment of wounds should be conducted on a more simple plan, without that elaborate attention to detail which Mr. Lister has thought necessary.

The discussion was again adjourned.

JUNE 2ND, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—73 Fellows and 15 visitors.

Books were presented by Dr. James Henry Bennet, Dr. Brickell, Dr. Bulkley, Dr. Tilt, St. Thomas's Hospital, the "Société des Sciences Médicales de Lyon," and the Editors of the 'Zeitschrift für Geburtshülfe.'

The following gentlemen were admitted Fellows of the Society :—Dr. Robert Cory, Dr. Walter Henry Drew, Dr. A. T. Gibbings, and Dr. D. E. Seton ; and the following were declared admitted :—Mr. Thomas Bailey (Godstone), Mr. J. H. Ewart (Manchester), Dr. James Gray (Glasgow), Mr. E. M. Sheldon (Liverpool), and Mr. Fred. W. Strange (Ontario).

The following gentlemen were proposed for election :—Mr. John W. Mason, Mr. John Powdrell, Mr. Matthew Reid, and Mr. W. K. Waller (Calcutta).

CASE OF SUPPURATING TUMOUR OF LEFT OVARY.

By Dr. J. W. J. Oswald.

J. Charlotte S—, æt. 40, governess, of Boxford, near Colchester, admitted October 21st, 1874, under Dr. Routh. Has not been well for six months. Came home 6th July. Had no cough, but great dyspnœa. Four months ago belly began to enlarge, and has continued to do so very rapidly ever since. During last three months great difficulty in walking; very little leucorrhœa. No catamenia since April last, but was regular up to that date. Formerly had dysmenorrhœa, not latterly, but a good deal of headache. Right lung dull, superior third, before and behind; respiration amphoric; heart-sounds weak; pulse about 100; anæmic, pale aspect. Walking induces dyspnœa and faintness. Cannot lie down flat or on left side, but can do so on right. Pains indicative of much anxiety, with lines at angles of mouth as in ovarian case. Perspires a good deal at night; cough very troublesome; makes very little water.

Examination.—Vagina large, uterus lying across it, and anteverted. On each side hard, elastic tumour. Both lumbar regions and upper part of abdomen clear on percussion. Generally fluctuation present. Abdominal swelling regular and uniform. Considerable pain over umbilicus, which is the seat of an irregular hardness, extending upwards, about the size of a fist.

Diagnosis.—Case seen by Drs. Savage, Rogers, Routh, Williams, and myself. It was believed to be possibly ovarian, but looking to its rapid growth, patient's emaciation, consolidation at right apex of lung, it was thought to be tubercular peritonitis. Dr. Rogers supposed it was cancer.

Treatment.—All measures of interference were discarded. She was well cared for and strengthened by good food, and sent home October 27th.

On November 3rd she died suddenly in the morning.

The following are particulars of post-mortem made by Dr. Gurdon, of Boxford, in company with his partner Mr. Mann, and forwarded to me :

Abdomen contained seven or eight pints of blood-serum and two ovarian tumours, the larger consisting of six or seven cysts, the smaller one on left side removed with the uterus. The smaller tumour was quite free, the larger adherent to the mesentery. The colon and smaller bowels were free from any inflammatory deposit. The lungs were not examined.

On receipt of the tumour from Dr. Gurdon I submitted it to a minute examination with the following results :

The tumour consists of two masses, with a slight constriction between them. The superior of these masses is about three and a half inches from side to side, two and a half from above downwards, and two from before backwards. The lower mass is about two inches from side to side, one and a half from above downwards, and one from before backwards. At the junction of these on the inner side are the Fallopian tube and broad ligament, which are normal in their size and character. The surface is dotted with hard nodules about the size of a pea, which, when cut into, seem to be composed of fibrous tissue. On the anterior aspect of the larger mass is a circular swelling, about one inch in diameter, and filled with venous blood. The peritoneal coat is thickened and easily peeled off. The tunica albuginea is thickened and pulpy. On making an incision a large quantity of sanious pus escaped, but there was no distinct cavity, the whole organ seeming infiltrated, the pus oozing from the tissue like blood after an incision had been made in an inflamed part. The parenchyma is congested and hyperæmic. The pustules full of pus, and in some places of white soft masses, which look like caseous masses. The

pulp is soft and easily torn. In the smaller of the two masses is an abscess with a distinct limiting wall; otherwise is the same as the other.

CASE OF EXTRA-UTERINE PREGNANCY IN WHICH A COMMUNICATION EXISTED BETWEEN THE CYST AND THE UTERUS.

By A. L. Galabin, M.D.

The patient, æt. 31, had had five children and two miscarriages. Her last confinement was in January, 1874. After that she was regular until the following June, when she supposed herself again to become pregnant. At the third month she had slight hæmorrhage, and at the eighth month, in February, 1875, she had severe pains and lost a good deal of blood. At this time the fœtal movements (which she had felt before) ceased. The breasts secreted milk, but afterwards became flaccid. She continued to suffer pain and to become gradually weaker until her admission into Guy's Hospital in May, 1875.

The abdomen was then found to be enlarged by a tumour reaching two inches above the umbilicus. It was quite symmetrical, and resembled in consistence a pregnant uterus. There was some fluctuation, but nothing like the limbs of a fœtus could be felt. *Per vaginam* the cervix was found to be central in position and hard, the os closed. Behind the cervix somewhat more than usual was felt of what seemed to be the body of the uterus.

It was at first supposed that the pregnancy might be extra-uterine, but it was found that the sound passed easily to the length of seven and three quarter inches, reaching

DESCRIPTION OF PLATE I,

Illustrating the Report on Dr. Galabin's paper on Extra-Uterine Pregnancy.

A. Uterus laid open by an incision through the anterior wall.

B B. Opening between uterus and extra-uterine cyst, bisected by the incision.

C. Vagina.

D. Portion of bladder.

E. Right broad ligament and Fallopian tube and ovary.

F. Placenta attached to posterior wall of extra-uterine cyst.

G. Umbilical cord.

H. Portions of cyst wall.

Dr Westmacott ad Nat Del. Jan 1875.

⅔ nat size.

W West & Co lith

into the upper part of the tumour. It was not tinged with blood, and no fluid escaped on its withdrawal. A laminaria tent was then introduced on May 14th in order to explore the cavity of the uterus. After twelve hours the obstetric resident found that the cervix would not fully admit the finger, but he passed a large-sized catheter easily into what was supposed to be the cavity of the uterus, and half a pint of brownish fluid, not much decomposed, escaped. The patient had some pains during the day and night.

On the 16th the discharge had become fœtid. The cavity was washed out with disinfectants, and several laminaria tents were introduced. At 10 p.m. the breech of a fœtus could be felt presenting at an opening resembling the internal os uteri, at the extremity of a long cervix, and somewhat beyond the point to which the tents had reached. The cavity was again washed out, but the patient had already become collapsed, and died at 4.30 a.m. on the 17th.

Post-mortem.—On cutting through the abdominal walls a greenish, semi-sloughing membrane was exposed, which formed an entire cyst containing the fœtus. The placenta was attached to its posterior part on the left side. There was a round opening from the cyst into the uterus, a little above the internal os, which admitted the tips of two fingers. Its edges were smooth, not like those of a recent rent. The opening was in a line with the cervix, and the body of the uterus was sharply retroflexed. There were signs of old and recent peritonitis.

On examining the specimen it might be a question whether the opening was due to the giving way of the uterine wall at the point of least resistance, or whether the ovum could have been originally implanted in a pouch in the uterus at the point where the opening now exists, a spot which appears to be quite away from the course of the Fallopian tubes.

INTRA-MURAL CALCAREOUS TUMOUR IMPEDING LABOUR.

By Dr. A. Wynn Williams.

On April the 19th Dr. Kirby requested me to see a patient with him who had been in labour for some time. He believed there was a bony tumour of the pelvis preventing the passage of the child's head, and considered it would be necessary to perform craniotomy.

The patient was forty-five years of age, primipara. On making examination the finger came upon a hard bony mass, covered with mucous membrane and apparently firmly attached to the ilium and sacrum in the vicinity of the left sacro-iliac sychondrosis, stretching across the pelvis, dividing the cavity, as it were, into two halves, leaving a space between the bony tumour and the opposite side of the pelvis of about an inch, where could be felt the os uteri, not much dilated, but readily dilatable, through which could be felt the child's head, and which was prevented making the necessary pressure on the os by the bony obstruction on which the child's head rested. I agreed with Dr. Kirby that it was necessary to perforate the child's head, which I did, chloroform being administered by Dr. Kirby. After having done so, and scooped out much of the brains, and removed several pieces of the parietal bones, I found it quite impossible to draw down the head, and came to the conclusion that our only resource was to procure a cephalotribe and crush up the child. Knowing that my friend Dr. Murray possessed one of these formidable instruments, I called upon him and asked him to come along with me and bring it with him, which he kindly did. On my return, after making an examination, I found the state of things entirely altered; the hard mass was not now firmly attached to the pelvis, but was moveable, and on inserting my finger into the os I distinctly

felt a rounded bony mass in the cavity of the uterus, whilst the head of the child had receded, and, in fact, the presenting part was now the tumour. I came at once to the conclusion that this mass had been contained in a pouch of the uterus, that it had been intra-mural. As, however, there was a possibility of its proving to be attached to the

child's head, and as it was certainly within the cavity of the uterus, it was deemed prudent, before attempting further delivery, to introduce the cephalotribe and crush up what could be grasped by the blades. This was done by Dr. Murray, and a piece of the mass was broken off and was brought away in the blades of the instrument.

After this there seemed space enough for the child to pass, and it was pulled through tolerably easily by means of the hooks. There still seemed some impediment to the passage of the placenta, and on introducing my hand into the uterus I came upon this large mass of calcareous matter, which I withdrew with my hand, the placenta immediately following. When my hand was in the uterus I could feel a pouch like a watch-pocket in the lower segment of the uterus, in which the tumour had been ensconced, in all probability for years. It was this intra-mural calcareous tumour wedged against the pelvis by the pressure of the child's head, and on which the force through the child's head was expended instead of on the os, that gave the sensation of its being a bony tumour attached to the pelvic bones. The lessening of the child's head removed this pressure, and the contractions of the uterus during my absence forced the tumour out of its nest into the cavity of the uterus, it being also not at all improbable that it was a perfect intra-mural calcareous tumour, and that during my manipulations and endeavours to extract the head of the child I had torn its thin internal covering, and in this way caused the enucleation of the tumour.

I think there can be no doubt, although there is no very marked history of uterine disturbance, that the patient must have suffered from an intra-mural fibroid for very many years, and that calcareous deposit had taken place in it, the advanced age of the patient (forty-five) before pregnancy took place having favoured this deposit. The tumour is composed microscopically of fine granular matter and ordinary fibrous tissue, as described in Paget's 'Pathology.' That fibroid tumours have been converted into calcareous there can be no doubt, but I never yet heard of one impeding labour. I am happy to say the patient made a good recovery.

CASE OF EXTREME DROPSY, FATTY DEGENERATION, AND FRIABILITY OF THE PLACENTA.

By John Brunton, M.A., M.D., L.F.P.S., L.M.,
COUNCILLOR OF THE MED. SOC. LOND.; F.O.S.; SURGEON TO ROY. MATERN. CHARITY, &C.

Mr. President and Gentlemen,—I beg to show you a very rare form of placenta. Unfortunately the process of preservation (in spirit of wine) has very much altered its condition. It is one of the unfortunate preparations which Dr. Tilt said were spoiling or spoiled.

When it was born it was fully four times as large, but the fluid contained in its structure has been absorbed by the spirit. The nature of this placenta is one of extreme hypertrophy (big enough for three children or more), and fatty degeneration coupled with general dropsy.

I was called to a lady in an hotel adjoining my house on Easter Sunday morning. I found her in labour with her first child. She had not gone the full term, but was in the beginning of the eighth month of utero-gestation. On examination I found an abnormal presentation, hand and foot (right hand and left foot). There was scarcely any bag of membranes. The patient was rather obstreperous, and, she being fat with thick thighs, I had some difficulty in manipulating, but by dint of perseverance I altered the presentation by bringing down the foot. By-and-by I delivered the child, which just gave a gasp or two and died.

On proceeding to remove the placenta I just caught the cord sufficiently to enable me to ascertain if the placenta was on the vagina, when without traction it broke off.

Cooling my left hand in water, I pressed the uterus through the abdominal walls, and was surprised to find the uterus very large and doughy. There was a little hæmorrhage, then action came on and this large placenta was extruded; further pressure, applied to deliver clots, delivered instead another portion of placenta, separate, distinct, and without membrane, then a second, and finally a

third piece. I put on firm pressure and a tight bandage; little or no loss followed, and for the first time I discovered that my patient was in an advanced state of dropsy. Feet, legs, thighs, abdomen, face, &c., swollen and pitting on pressure. When I was called it was neither night nor day, and in the dimness of the morning light I did not notice her condition. I have examined her urine; it is highly albuminous. She has done extremely well.

I believe this condition of placenta (dropsical) to be rare, and its friability I think is also remarkable. It becomes a question of importance to ask, when did these pieces break off? If they broke off early, then one would have expected concealed accidental hæmorrhage. There was none of this, and it is probable that uterine action severed them. Had I not carefully extruded these pieces, or had I been content with the delivery of the first placenta, in all probability a condition of affairs would have been left such as would bring on an autogenetic case of that fever which has been the subject of discussion for several nights.

CASE OF MONSTROSITY.

By Dr. Wallace.

The fœtus exhibited was born on the 21st of April, the mother being a primipara. The presenting part was the tumour on the head, which, when full of fluid, quite filled the cavity of the pelvis, and thinking it a case of hydrocephalus I tapped it, and the labour was easily completed. It was then found that the tumour, which seemed separated from the head, communicated with the brain; that one eye was wanting, and the left eye only rudimentary; there is no tongue, and the lower jaw seems scarcely developed, and the lower part of the face is connected with the skin over the clavicles by cicatricial-looking bands; there are six fingers on each hand, and

six toes on each foot; the feet are both turned inwards, and the sex of the child cannot be thoroughly made out. There is no history of syphilis.

Dr. Wallace exhibited the uterus of a woman, æt. 55, who had had two miscarriages at the age of thirty-nine, she having been married the previous year. Shortly afterwards she noticed a swelling in her abdomen, and was tapped several times at different institutions, when in the year 1868 she came under my care as a parish patient, and I tapped her quite a dozen times during the following eighteen months, drawing off a very large quantity of fluid each time; in fact, the operation was not performed till the dyspnœa was so great as to render it absolutely necessary; she had no uterine symptoms during this time, and it was thought to be an ovarian tumour. I lost sight of her till I heard that she had died in the Holborn Workhouse last month, when, through the courtesy of the medical officer, I made the post-mortem. I may say that she had not been tapped again, and there was only a small quantity of brownish fluid in the peritoneal cavity. She had had no hæmorrhage for twelve months, and no great loss at any time. She died of subacute bronchitis.

PLACENTAL POLYPUS.

Dr. T. C. Hayes exhibited a specimen of placental polypus taken from a patient, on eighth day after delivery, whose labour had been natural and easy, though followed by a smart hæmorrhage. Placenta was said to have been entirely removed, and patient seemed to go on fairly well till eighth day, except that her pulse was quick and she was sleepless.

On eighth day, when seen by Dr. Hayes, extremely ill; had had a rigor, intense headache, delirium of muttering

character; pulse very feeble and compressible, 120; temp. 105·4°; no abdominal tenderness; in brief, she had all the symptoms of severe septicæmia.

The placental polypus, about the size of a hen's egg, was felt through the os uteri, and speedily removed under chloroform. The lochia were horribly putrid. The uterus was carefully washed out with a solution of Condy's fluid, and opium given to procure sleep. The next morning, temp. 99·1°; pulse 108. The injection of uterus was continued twice daily for three days. The patient made an excellent recovery. The polypus was pear-shaped, and surrounded by a layer of blood-clot.

The discussion on puerperal fever was then resumed by—

Dr. Savage.—I was quite prepared to commence the discussion this evening, but as I hear with great satisfaction that we are favoured with the presence of Dr. Farre, I should much prefer to listen to any observations that he may make.

Dr. Arthur Farre.—I am very sorry if I have disturbed the order of the discussion, but I rise in obedience to the invitation conveyed to me by you, sir, and, as I understand, by the Society at large, to take part in the discussion. As an honorary Fellow of this Society, I thank you for the compliment you have thus paid me. It has occurred to me to think, what are the principal objects which the Fellows of this Society had in view in promoting this discussion? Hardly, I should think, to elicit anything very new from any of the speakers; but the purpose probably was, that we should compare our observations, that we should, as it were, take stock of our knowledge, and incite each other to further inquiry and investigation; and with this idea I have come to-night

to take what little part I may in this discussion. In speaking of taking stock of our knowledge, I have it in my mind that we should not only compare our observations together as men of the present day, but that we should also take into account the labours of men who have long preceded us. I have no doubt that the Fellows of the Society have in their recollection those excellent works of such men as Denman, Leake, Hull, and various other workers at the end of the last century, who gave their attention to this subject. I have no doubt that a great deal of information might be evoked from a re-perusal of the works of those men. Actuated by this idea, and looking into some of these works, I have been much struck by the observations of Dr. Kirkland upon this subject in his most interesting essay, which I have no doubt is well known to the Fellows of this Society, on *Childbed Fever*. Dr. Kirkland commences his essay by inquiring "what is properly a puerperal fever;" and here he seems to open up the very subject we have met to discuss to-night. It is interesting to observe how a man more than a century ago took in all the points which we have now met to discuss, or most of them, and valued much in the same way as we now value these very same questions. He begins, for example, by admitting that every disease happening to a woman in childbed, connected with the act of parturition, may perhaps be properly termed a puerperal fever, though some writers use this by way of eminence to distinguish that form which they think most dangerous from milk and other forms of fever connected with child-bearing. Having admitted this, he proceeds to offer the following objections:—He says, if by using the term puerperal fever these writers had chosen to fix upon any particular form, it might have answered a good purpose; but he does not see what good will arise from combining such diseases as may happen to a woman in child-bearing, as inflammation of the uterus, or putrefaction of clots within the uterus, producing fever, or a fever engendered from without by the admission of

atmospheric poisons. Then he adduces several such instances as the proposer of this question has adduced. He mentions the case of smallpox occurring to a woman in childbed, hospital fever, and the like; and then he discusses the propriety of considering these as puerperal fevers at all. He points out very accurately that, when we give a specific name to a disease, the name ought rather to indicate the disease than the condition of the individual in whom it occurs. Then he makes another very excellent observation, which I think we should all keep in view, namely, that when we speak of fever we are not speaking of a disease, but of a sign, or a set of signs, indicative of a mischief going on within. He then sums up by saying, "In short, the writers in the present day seem to include under this name of puerperal fever a thousand disorders." Now that, I think, is as nearly as possible in accordance with the views of the Fellow of this Society who has brought the matter under our consideration to-night. He objects to the term "puerperal fever," and thinks that we ought to distinguish the different diseases which have been classed together under this title. I suppose he had in mind some such idea as that which actuated Lord Bacon in reminding us that we are all too much in the habit—neglecting things—of worshipping specious names. I suppose that, in the use of the term puerperal fever, we have for a long time been worshipping a specious name, and not considering sufficiently the thing which that name indicates. Having asked your attention to the works of this author, and shown how far his idea seemed to be in a measure coincident with those which are now under discussion, I would proceed to refer to a few of the circumstances connected with the definition of puerperal fever which has appeared in the work known as the 'Nomenclature of Diseases,' emanating from the College of Physicians. I should not have done this if the author of these questions had not done me the honour to mention my name in connection with the part which I took in the Nomenclature Committee of the College of

Physicians in respect to this matter, as one whose business it was mainly to suggest to that Committee the various definitions that should be accepted in regard to those subjects that come particularly under my department. I think some little misapprehension must have arisen as to the objects of the Committee in framing that definition and offering it for the consideration of the profession. In order to make this clear, perhaps you will allow me to call attention to one or two observations that are made in the introduction to this work. Few men, I believe, read the preface to a book, and it may be that many of the Fellows of this Society have not read the few observations that precede the catalogue of diseases known as the nomenclature of the College of Physicians. They begin by saying—"The object of this nomenclature is to aid in perfecting the statistical registration of diseases, with a view to the discovery of statistical truths concerning their history, nature, and phenomena." Beyond this they do not profess to proceed. They then call attention to this fact, that whilst, generally speaking, the name of a disease only is given—as smallpox, for instance, which requires no explanation—there must naturally occur some that would seem to require explanation; and to those they have added a definition. But then they distinctly say that those diseases only have been defined which seem to require it; and the definitions have been framed for the purpose of identification only, not as an explanation of the phenomena of disease. So that, when a definition is offered, it is offered under these circumstances; and there is no attempt at dogmatic teaching, or entering upon any other considerations than those which would enable practitioners, whose business it was to register diseases under their respective titles, to identify the disease that was intended. I think it is as well to keep this in view with reference to the observations made upon this subject by the proposer of the questions. Now, I would like for a minute to draw attention to this definition of puerperal fever here offered. It is defined to be "a continued

fever communicable by contagion." I ask your attention for a moment to the word "communicable," for there was a good deal of discussion upon it. It is simply meant to convey the idea that the disease may be communicated, or is capable of being communicated; but that it does not necessarily arise in that way—that it may have a more spontaneous origin. "A continued fever communicable by contagion, occurring in connection with childbirth, and often associated with extensive local lesions, especially of the uterine system." That word "often" was also made the subject of comment; and I wish to observe, in connection with what has gone before, that the object of introducing that word was simply to call the attention of those who would have to register disease to the frequency with which these complications arise; there was no intention of entering upon any doctrinal question, or expressing any views as to the precise nature of the disease under consideration. Then there is added a note in which those who register diseases are requested to return under the name "puerperal fever" the more important local lesions, such as peritonitis, effusions into serous and synovial cavities, and the like. That is done for the same purpose, so that nothing might escape observation, and that every circumstance connected with the disease should be carefully recorded for the purpose of collecting facts. I will now allude to the place which this term puerperal fever occupies, because I observe that it has excited much attention. It is not placed among the fevers at all; it is placed among the general diseases, but it is placed far away from the fevers. First of all come the infective fevers—small-pox, cow-pox, chicken-pox, measles, and the like; then, lower down in the list, follow pyæmia, erysipelas, &c.; then comes puerperal fever. It was placed in this position because the Nomenclature Committee were entirely at a loss to know where it ought to come in the catalogue of diseases. It found a place of refuge at last at the bottom of the list, after pyæmia, erysipelas, &c.; and the Committee had it in

their minds to indicate by this place which they assigned to the disease the apparent connection of puerperal fever with those antecedently named diseases. I would further call attention to the fact that, independently of this, there is in the nomenclature a list of those affections which are consequent on parturition; and this is quite apart from the subject which we have now been considering. Now, in connection with the same subject, I would ask the Fellows to consider if there be any objection to this term puerperal fever. What is meant by the term fever at all? What are our views with regard to fever? Do we consider fever as a disease to which we ought to attach a special name? or is it only a symptom (as Dr. Kirkland long ago reminded us) of a disease? Do we regard fever —that is, the pyrexial action—as anything more than an indication of an underlying disease? If we keep this in view we shall find the difficulties that continually occur in framing a nomenclature when we take the name of a disease to indicate that which does not signify its nature. These few observations I thought it right to offer with respect to this part of the subject. And now, as each of us is expected to answer to the best of his ability the questions put to us, I will take them in turn. But first I must confess that, in approaching the subject, I feel a great difficulty on account of the manner in which it is brought under our notice. I observe that these questions, which only profess to be questions, assume rather the form of an argument; they are rather put to us in a syllogistic form; and they are intended to lead up to a conclusion, though no conclusion is stated. Obviously, they are questions merely, and they are left to us to consider. But, if we are to follow the order of these questions, I must say it is with some difficulty that I approach the subject, inasmuch as that which we have to deal with is associated with a great deal of imperfect knowledge, and we cannot precisely formulate all the facts so easily in the shape which is offered to us. Still, with this difficulty before me, I will endeavour to the best of my ability to

notice each of these three questions. The first, as you well know, relates to this: Whether there is any form of continued fever communicated by actual contagion or infection, in connection with childbirth, which is distinctly caused by a special morbid poison, and as definite in its progress as the diseases which are taken in illustration. To that question I can return a very plain answer, and say that I am not aware of any form of contagious or infectious fever connected with childbirth which is distinctly caused by a special morbid poison (if by that be intended a specific virus), and which has a definite progress. I am not acquainted with such a disease. The next question and the following one must, I think, be taken together. "May all forms of puerperal fever be referred to attacks of some infective continued fever — as scarlet fever or measles — occurring in connection with childbirth, on the one hand; or, on the other, to some form of surgical fever, or to erysipelas, caused by or associated with changes in the uterus and neighbouring parts following the process of childbirth?" I am unwilling to give an answer to this question, because I wish to keep open another subject, which seems to be closed by the question which follows it. If I were to give an answer to that question, I should be shut out entirely by the form that is laid before us from giving a satisfactory reply to the third, which is this: "If all cases of contagious and infectious diseases which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?" Now, it is to the form of this question that I wish to take exception; because it appears to me that, in this question, the several infectious and contagious diseases are treated as if they occurred in the body of a man, a non-parturient woman, or a child; whereas I wish to keep together the connection which associates them with the act of parturition, and shows them as occurring in the parturient woman; and I think it is the separation of these two conditions that occasions the difficulty I now

encounter. I cannot, consistently with my experience, agree to consider these diseases as one and the same when they occur to the non-parturient woman and the parturient woman. I think here there are several circumstances that have to be taken into account. In the first place, when any of these attack a parturient woman she is not in the same condition as a healthy person whom they may attack. Scarlatina, measles, and the like, occurring to healthy persons, do not enter the body of those individuals in the same conditions as they enter the body of a parturient woman. Let us remember, in the first instance, she has been in some degree weakened by the previous confinement. It may be said, "Well, but in some cases we have a parallel—cases in which the diseases here mentioned follow after an operation." But I will go further. We have two processes going on in the body of the lying-in woman naturally, which do not occur in the person of one ordinarily struck down by one of these diseases. First, I refer to the milk-process, the milk-secretion, and to that attendant disturbance of the constitution which we term milk-fever. This is one element to be taken into consideration, and one disturbing force. And there is another and a much more important one, and that is the changes which go on in the uterus, and which we all know as the involution-process. Nothing of this kind occurs in the body of an ordinary recipient of those diseases. This natural process, as we know, goes on in the body of the woman at a time when she is most likely to be attacked with the diseases that we have met to consider; and when any of these diseases attack a parturient woman this process is materially interrupted. Now, we know that when this process is interrupted there is an arrest of all that eliminative action which that evolution-process implies; and we may fairly conclude that, in consequence of this arrest, there is some accumulation in the body of those effete matters which ought to be expelled from the system; and this, added to the already existing blood-dyscrasia, must very much aggravate the disease. Nor is

it quite certain that this may not add a new form of sepsis to that which is already in the blood. Before admitting, therefore, that there is no such thing as puerperal fever, I would say that all these things ought to be taken into account. It has been thought by some that such a thing as a pure and independent puerperal fever may be found. I should be sorry to shut out that idea altogether as one that is impracticable, and not to be entertained. I think it is very easy to see where we shall find a puerperal fever, if we are to look for it at all. I mean a fever not caused by any of the circumstances here suggested to us; it is quite plain that we must look for it here, in the arrest of some of these processes that I have been referring to. If we are to look for a puerperal fever at all, once that can be properly so called, a disease which is *sui generis*, we shall probably find it in the interruption of these healthy processes. Then, if I am not trespassing too long upon your attention, I should like to express a little more nearly my own views with regard to puerperal fever. I very much regret now that, when we were framing this definition, it did not occur to me to suggest that the term "puerperal fever" should be rendered "puerperal fevers;" and that a note should be placed to the effect that, under this name, is intended to be comprehended a class of continued fevers communicable by contagion occurring in connection with childbirth, and so on. I think we should have got over a good deal of the objection taken to this word, if we had considered this not as a special form of fever, as it appears to be when it occurs in the nomenclature, but as a group of fevers connected with the act of childbirth. If I were asked how I would classify the diseases that we have been in the habit of grouping together under the name of puerperal fever, I would divide them into these three, as it appears to me, very natural classes. We have to look at this matter in a practical light, and see how we shall consider this subject, not by the teachings of the dissecting room, but as we see it in the lying-in chamber, when

asked, as we constantly are, to determine the nature of the disease that we are called in to examine and advise upon. I have been in the habit of dividing all cases that can in any shape be termed puerperal fevers into three classes. First, those simple fevers which may, for the want of a better term, be called irritative fevers; and under that name I would class that febrile action which results from simple mammary irritation, and is known as milk-fever; those simple febrile consequences of a traumatic origin which result from slight injuries to the soft parts, laceration, and the like, though those are of rare occurrence; and those pyrexial states which are of a fugitive and transient nature (I think, in a second catalogue in the nomenclature, we call them puerperal ephemera). First, then, those irritative fevers arising from some local irritation, and not implying any blood-infection of any kind. In the second class, I would place those infective fevers which are not of a specific origin, and in which the poison or sepsis does not undergo a distinct period of incubation, though I have no doubt there is a poison conveyed into the blood, in what way it is not necessary now to consider. Under the name, then, of the milder forms of infective fevers I would class all those in which the infection is not of a specific nature, in which the process does not undergo a period of evolution, a period of development, and in which the consequences follow no definite order. Then, in the third class, I would include eruptive fevers, and those which depend upon a specific blood-infection, the poison following a specific course, having a regular period of incubation, and terminating in those several diseases, eruptive fevers and the like, which occur, of course, to the lying-in woman in common with others. These I do not consider as in any way taking any part in the puerperal fever. The only fevers that I would acknowledge as in any way connected with the puerperal state are the first two. I would entirely exclude the others from the catalogue; but, in the present imperfect state of our knowledge on these

questions, I would like to retain these diseases under some name corresponding to that under which we now group them together. If I proposed any alternative, I would change the name. One that I am in the habit of employing is *post-partum* fevers, I have long discarded the name puerperal fever, and have used the term *post-partum* fevers, which implies no theory at all, but simply expresses the fact that they occur to women after delivery. I think that, under that head, we might include all these three forms of fever. If, for the purposes of statistical registration, it was desired to know how these names should be registered, I would suggest that those cases should be registered under a separate head in connection with childbirth, never mind in what relation they stand. I would place, for instance, "scarlatina" and "scarlatina in the puerperal form" in parallel columns; "erysipelas" and "erysipelas in the puerperal form." In this way, we should satisfy the requirements of the Nomenclature Committee, and not foreclose that most important question that we have now under discussion. I dare not trespass longer upon your time; I cannot venture to offer any further observations upon this subject; but I may at some future time, if you please, further develop my ideas, and particularly call the attention of the Society to some circumstances, which appear to me to have been left too much out of consideration. It seems to me that, in considering the pathology of these cases, we have given too much attention to the influence of a blood-poison, and have lost sight of that intermediate condition—or, rather, we have not attempted to determine it—which intervenes between the entrance of the poison into the blood and the development of it in the various ways and forms which give significant names to the diseases under which they are classed. I think also we have too much left out of consideration the influence of the nervous system, the influence of these poisons upon the nervous centres; and I think if we were to direct our attention further to this particular, we should be able to throw a great deal

more light upon these diseases, and, perhaps, get over some of the difficulties that now surround us.

Dr. Savage.—I feel under some embarrassment after hearing Dr. Farre. I certainly came prepared with very decided views, but they have been somewhat shaken by what has fallen from that eminent authority. With regard to these questions of Mr. Spencer Wells, I do not know that Dr. Farre finds any more difficulty in dealing with them than the rest of us. Evidently, the object is to catch us at every corner, so that we might not escape from the subject in any way. I can scarcely agree with Dr. Farre in leaving the subject in so uncertain a state as he proposes. We have already had, in the course of the last two discussions, some positive statements made to us which touch closely on the real nature of the complaint. I was at an early period prepared to argue strongly against any pathological relation or affinity between fevers and these under discussion; but I listened attentively to what fell from Dr. Squire and some of our country friends, who sustained so well the argument on that point that I think it scarcely necessary to trouble the Society with any observations of mine. I only wish to say that there seemed to be, on the part of Dr. Barnes and Dr. Braxton Hicks, a clinging to the opinion that there was a sort of connection between scarlet fever, or the sort of scarlet fever that they had seen, and puerperal septicæmia. It could not be the orthodox scarlet fever, because it is clear that you can have in a parturient woman right down scarlet fever, and she will get over the scarlet fever and the parturition exceedingly well. I suspect these gentlemen mean some other form—a bastard form of fever—that sort of thing spoken of by pathologists, in their new vocabulary, when they say that a thing exists "potentially," and not actually. I will not criticise anything that has fallen from these authorities; I will only say that I find a difficulty in recognising the presence of any disease in the absence of its distinctive

symptoms. Now, is this fever in any sense of the word? First, we have decided that it is utterly distinct from such things as we have called fever. Is it fever? We all agree that there must be some heat of the skin, some elevation of the temperature in anything that you can call fever; now, I know that you may have many of these fatal cases of septicæmia in a parturient woman running their course in eight days, without any elevation of temperature from first to last; or it may be that there is an elevation on the second day after you discover the presence of the poison. I agree with Dr. Farre that a great deal may happen before you discover the presence of the poison. It is not impossible that, if we kept our eyes open and watched, we might anticipate the effect of the poison; but this is seldom done—we see the cases late, and they are generally fatal. In the matter of temperature, then, I think it is clear that the disease should not be called fever at all. Then, the next point is, whether it is septicæmia peculiar to a parturient woman, not like any other septicæmia. I have seen septicæmias after operations on silly women who are not intended to have a family, who are intended to be barren; septicæmias from the introduction of that most treacherous of all things, the uterine stem; septicæmias from legitimate operations upon the uterus; septicæmias after ovarian operations. I have seen all these, and I have also seen (but I am no authority on that point) surgical septicæmia in wards, and I can distinguish no difference between them and septicæmia in parturient women. I think it to be a simple septicæmia—that is, a disease which you must set apart from every other sort of disease likely to occur in parturient women, such as the disease you call fever in connection with the secretion of milk, and other fevers. It stands quite apart and alone, and I think it of great importance that we should consider it as such. I think, if we do not, we shall be likely to overlook it in our patients; and, instead of applying, as we might with some effect at an early date, some remedy, we may let the case go on

until it is past hope. Now, do we know anything of the poison? Have any of us seen or touched or felt it? Mr. Wells will tell you by and bye, if he please, that in the course of ovarian septicæmia he sometimes taps the peritoneum with great effect, and lets out a great quantity of it. There must be poison in or about it, because the woman gets better when it is let out, gets worse again when it accumulates, and again better when it is let out. Then, there are the processes, which it would take too long to enter into, such as washing the peritoneum and the like. If the case be taken in time, the woman gets well. Evidently, in such a case we are dealing with a poison in some shape or other. Is the whole fluid poisonous, or is there something special in it? Our German *confrères* have helped us a good deal in this way, in a negative sense. The ordinary results of putrefactive changes in dead matter should be carefully distinguished from the changes which occur in fermentation; they are two different things, but are often confounded. We have, in the last, sulphuretted hydrogen, sulphuretted ammonia, and butyric acid; there are other things, no doubt, but these have been discovered. I am referring to the experiments of Billroth and Weber. They take a solution of each of these things and throw it into the cellular tissue of dogs and horses, and produce in each instance septicæmia, the animals dying in the usual way. You could not tell whether they had chills or not, still they died, and it was clear that they were killed through septicæmia. If you inquire as to the cause, what shall we answer? I must not forget to say that sometimes the severity of the symptoms did accord with the offensiveness of the liquid. I have had my fingers in the liquid, and could not eat any dinner for a week or two; and I have no doubt that if I attended a parturient woman with my fingers in that state she would have had septicæmia. In the case of the peritoneum, it is clear that this question can be answered. What are the channels for the introduction of this fluid into the system? Unless

the peritoneum is inflamed, it is clear this must take place through the absorbents. There is an instructive experiment by Dr. Sanderson, in his beautiful book lately published, in which he shows that liquids may possibly get into the system through the vessels, but what he calls particulate poisons cannot—they must enter in by the absorbents. I do not exactly know what he alludes to. I must refer gentlemen who wish to know more about it to the book itself. It is quite clear, then, that in the case of the peritoneum it must enter in through the absorbents—that is, if it be particulate, because it is asserted by the more recent pathologists that all septic matter is particulate, which, I think, is a mistake. I am sure one of Billroth's experiments shows that it is a mistake, because a solution of sulphuretted hydrogen cannot be particulate. Let us apply this to the uterus. We are first in some difficulty unless you acknowledge the existence of endo-uterine absorbents such as there are in the peritoneum. I think these absorbents are denied; Dr. Farre seems to deny them in his book, but, then, that was written long before Robert Lee's discoveries. They are denied also in a recent communication to our 'Transactions.' Still, they do exist. I make this remark the more because there was a theory started by Dr. Graily Hewitt the other day (as I saw by the report) about what he called the burglar-theory; that is, he said he had no doubt that in the majority of instances there was an imperfect contraction of the uterus, a clot within it—clots within the veins—and, the locks being taken off the doors, and the doors thrown wide open, the thief stepped in. In the first place, I can scarcely imagine such a state of the uterus without considerable hæmorrhage. He also spoke of sinuses of the uterus. Now, there are no sinuses of the uterus in my opinion. I do not know of any open sinuses after parturition at all. I know it is exceedingly difficult in the dead subject to separate the placenta from the uterus without tearing into the veins, but nothing is so easy as to separate the placenta in placenta prævia;

you come in contact with no large veins; you feel the small arteries, veins, and absorbents yielding; you separate the whole, and there is the uterus in a dilated state, and no hæmorrhage. That could not be if there were open sinuses. Even if you have got clots in the veins, it is a matter of demonstration, since Virchow's discovery of the true mechanism of embola, that clots in the veins do not get into the circulation by themselves—they never break up. That was the old opinion of Dr. Lee, whose name has not been sufficiently mentioned except by Mr. Hutchinson. Dr. Lee showed incontestably that one of the concomitants of this disease, septicæmia, was a clogging of the veins; but it was Virchow who demonstrated (it was quite a demonstration) that the clots will not break up; that is not the way in which the clots disappear. Books on pathology, especially German books, describe how the veins empty themselves, but time will not permit me to refer to that. We have arrived at this, that you cannot get the poison through the veins. We have, then, to do with another channel, the lymphatics. Now, the lymphatics have been shown by Leopold to be very numerous in the mucous membrane of the uterus; the surface is highly absorbent, and the small vessels of the uterus are invaginated in these lymphatics. This very curious arrangement is shown in Leopold's book. I fancy I have seen them. I do not say anything as to the microscope, since I find a power of fifty talked of, whereas I found a power of twelve difficult to manage. It seems to me, then, that as a matter of demonstration we have brought it down to this—that we have seen the septic matter, we have touched it, and smelled it; I have mentioned experiments in regard to the composition of it; and we now can have no difficulty in believing that some septic stuff will accumulate in the interior of the uterus and get into the circulation, as it did in the case of the peritoneum. Now we come to the difficulty about the fingers; I have already said that if I attended a woman in her confinement with my fingers in

that state I had no doubt she would have septicæmia, and I really have no doubt of it. You remember that we had some painful statements from a gentleman in the country who seemed to have had his fingers in that condition, and who lost case after case, and was obliged to give up his practice. I know it is difficult for most of us to comprehend how it is that the mere approximation of a finger should set going this curious septic thing. Now, every old woman will tell you that if she puts a piece of fœtid meat in the cupboard, though it does not touch the rest, all will be turned in the morning. The other day, when passing a butcher's shop in a large district, I found him hard at work with all his men. Having these perplexing questions in my mind, I walked in to hear what sort of disinfectant he used, and he said, "I use no disinfectant; I wash everything with soap and water every night, hooks, cleavers, knives, and everything, then I admit the meat; if I did not, it would all be turned towards the morning." I do not myself profess to understand or explain how it is that the approximation of bad meat to good will turn the good bad, but so it is, and we can apply that fact, though I have no explanation of it, to show how contact with a fœtid finger would lead to septicæmia in a woman. Then we come to the question of pyæmia and bacteria. Now, the question of bacteria has occupied the minds of leading members of our profession during six hours, at three meetings, in a sister institution, where they all contradicted one another, and arrived at no conclusion, but made shipwreck of the whole affair. Nevertheless, we have got to entertain it. Why did Mr. Wells ask us about pyæmia? What is pyæmia? A woman after her confinement has a pain in the upper part of the thigh, and there is a little swelling; you think there must be an abscess; you open it, and you let out a pint of matter. In a week's time there is a similar swelling in the calf of the leg; you open that, and there is half a pint of matter; and then she does well. Another woman with septic symptoms dies suddenly with

clots in the heart, and numerous abscesses in the lung. Another has not only clots in the lung, but has pus in the kidneys, a breaking-down of the spleen, and concretions on the valves of the heart. Now, Cruveilhier long ago injected into the medullary cavities of dogs, quicksilver, and they died with all the symptoms of septicæmia; then he found numerous abscesses in the body, and in the centre of each a small globule of mercury. I mention these facts in relation to the question of pyæmia. I should like to know which you call pyæmia and which you do not. In the case of a woman with an abscess in the calf of her leg you would call it pyæmia; but if you have any septicæmia with it, you would call it septicæmia; yet the source of the pus is the same; the pus is nothing but dead leucocytes in every case. I am inclined to dismiss pyæmia, as I would puerperal fever, entirely from the catalogue; and I believe that Mr. Callender, whom we heard the other night, has the same feeling. He seemed to me to hesitate very much as to the term pyæmia. I do not believe that pus is ever in the blood as pus, but as living leucocytes it is, but then being living it is not pus.

Dr. Braxton Hicks.—I have never said that there is distinct scarlet fever producing puerperal fever. If Dr. Savage looks at my paper, he will find it stated that it might permit a condition which we call puerperal fever to come on; and, as a clinical fact, I have no doubt of that.

Dr. Savage.—It seemed to me that Dr. Hicks and Dr. Barnes had an idea that there was some connection between the two, or between some form of scarlet fever and some form of puerperal fever. I say again, it would not be real scarlet fever, because we have had testimony that you can have scarlet fever in very severe form, and the woman will do well; therefore, I could not imagine that regular scarlet fever was meant.

Dr. Barnes.—I also wish to disclaim the interpretation

put upon my words, and I refer the Society to what I have written on the subject.

Dr. Wynn Williams.—In answer to Mr. Wells' first question, I can only say that, in my experience, I have never met with a case of so-called puerperal fever existing as a disease *per se*. I have seen many cases of disease described as puerperal fever, with all the characteristics, which I have been invariably able to trace to a cause easily recognisable, and capable, in most cases, of being recognised early enough for removal; and, as a matter of course, I do not believe that the disease is due to any special morbid poison. In answer to the second question, I am also decidedly of opinion that puerperal fever cannot be referred to any special disease, although the exciting cause of puerperal disease may arise during the progress of any one of the diseases alluded to, and many others—that is, should they happen to be accompanied by any gangrenous or suppurating wound. Women are often confined with the diseases mentioned in the same room with them, and derive no injurious effects therefrom. Again, puerperal women exposed to those diseases, and who have not been previously attacked with them, will take them, as other persons, without any special action on the uterine organs. I could relate numerous instances proving both these statements, did time permit. I shall consider the third, fourth, and sixth questions together; the one is so intimately connected, practically, with the others. I have stated what I believe the disease not to be; I will now state what I believe it is. Every case of so-called puerperal fever—I do not mean peritonitis and such like—is due to septicæmia, to septic contamination of the blood; and its injurious effects are first and most severely recognised in the wearied and bruised uterine organs and surrounding parts. The disease, then, is purely septicæmic. I agree in the main on this head with what fell from Dr. Graily Hewitt, with the exception of his terming the disease pyæmia.

It is not pyæmia, and I consider that the calling of it so has led to much of the erroneous doctrines that have been promulgated. For instance, a female, three or four days after her confinement, is taken with what is termed puerperal fever, and rapidly sinks in a few hours. It is said she died of pyæmia. A *post-mortem* examination is made, and no pus is found, only a quantity of bloody sanious serum, with a peculiarly sickening and offensive odour. This is the poisonous matter. If the patient had not been overpowered, as it were, by the large doses of septic poison in her system, and had possessed sufficient strength and vitality to form pus, she would have had a far better chance of recovery. I look upon the formation of pus as a healthy effort of nature to surround and isolate the irritating poison. In fact, I do not believe that pus—laudable pus, as it used to be called, whether in or out of the circulation—ever killed any one; it is only injurious when it becomes putrid. I believe that putrid animal matter acts injuriously on the puerperal female in two ways, and that the symptoms vary both in intensity and character. In both instances the septic poison enters the system; but in a different form; and as in the one case the poison is much more concentrated than in the other, so its effects are more overpowering and rapid. In the case of most intensity the poison enters the system in a state of solution; in the more chronic cases it enters as vapour In the first case there is a breach of surface, generally in the vagina; in the other there is no breach of surface. Where there is a breach of surface the septic poison comes immediately in contact with the fresh wound, and a dose sufficient to knock the patient down at once is taken into the circulation; in the other case a mass of putrid matter—it may be decomposing blood—is pent up in the uterus or vagina, and penetrates the mucous membranes and deeper tissues more slowly; in fact, it is a case of continuous poisoning. This form frequently terminates in puerperal mania. There is, then, no such disease as puerperal fever; it is nothing more nor less

than septicæmia, and is always due to the presence of putrid animal matter. Now, this putrefaction may be caused in three ways: by retention of portions of the placenta or blood-clots long enough for them to undergo decomposition; by the bruising and sloughing of vaginal mucous membrane; and, thirdly, where the septic poison is conveyed to the discharges of the patient, and by its presence sets up rapid putrefaction in the lochia—so rapid that, when once the fermentation is set up, the whole of the lochia appears to become putrid at once, like the action of yeast on wort and dough. The first is to be prevented by not leaving any portion of the placenta or of blood-clot (as far as practicable) in the uterus or vagina. It must not be overlooked that certain conditions of the atmosphere also tend very materially to promote this putrefactive fermentation. This has been described by Sir J. Paget, who has pointed out that at times the wounds of patients, not only in hospital but outside, take an unhealthy character and become gangrenous. Of course, should any portion of placenta or blood-clot be present, your first care will be to remove them, and wash out the vagina, and, when necessary, the cavity of the uterus, with some antiseptic fluid. You must not be satisfied with telling the nurse to do it, you must see her do it, or (as I generally do) do it yourself the first time. The treatment of the second form, when you have a slough, is the same—to syringe out frequently with some antiseptic fluid. To ward off the third cause, every care must be taken to prevent any septic or putrid poison from coming in contact with the lochia. I have not time to point out how this may be best attained; neither is it necessary, as you have already heard all that can be said on the subject, as to isolation, ventilation, and the like. No method, however, has been pointed out, or even alluded to, in the course of the discussion by which the septic poison could be got rid of should it have attached itself in any way to the body or dress of the accoucheur or nurse. We have more than one substance

—one in particular—that will not tolerate the presence of putrid animal manner or septic poison; this drug is iodine, which, as soon as it is brought in contact with septic matter, is converted into two harmless substances, hydriodic acid and carbon.* Let any one who has been in attendance on a case, whether a parturient female or otherwise, where there is any putrid emanation, wash his hands in water into which he has poured tincture of iodine, and I will answer for it he has no septic matter under his finger-nails. Again, if he fancy that his clothes, hair, and skin are saturated with it, let him go into the water-closet (I mention that as the smallest room in the house), place a few scales of iodine on a plate and put a spirit-lamp under it, and he will soon find himself surrounded by a violet vapour, which will fall upon him in a shower of minute scales, from which he has only to protect his eyes. If he then carry with him any of the puerperal poison, my whole theory of the disease must be wrong. One thing I can assert, that in my own practice I have never had a case of fatal puerperal septicæmia since I have used iodine as an antiseptic, now more than twenty years ago. Iodine is equally efficacious in warding off septicæmia in other surgical diseases. I have injected solutions of septic poison under the skin of guinea-pigs, and produced death by septicæmia. I have also injected some of the same solutions, into which I had dropped a few drops of the tincture of iodine, without producing any ill-effects. It is needless to observe that the prevention and the cure of this disease go hand in hand together. Should any septic poison be present in the puerperal woman, wash her out again and again with solution of iodine until the solution comes back the same colour as it was thrown up. The temperature of the patient will probably be 103° or 104°, and it will go down in a very short time to 98°. This I have witnessed since the present discussion began. The fifth question is as to

* *Vide* article "Septicæmia," 'St. Andrew's Medical Graduates' Association Transactions,' vol. i, p. 98.

bacteria—a delusion and a snare. It is possible that the septic poison may be conveyed by them as by any other substance floating in the air that is called harmless; that is all the harm they can do, as I take it; they live on this putrid matter as mites and maggots on cheese, or vibrios on decayed potatoes during an epidemic of potato-disease; in fact, I am inclined to think that they are present for the purpose of removing the poison, not generating it.

Dr. Playfair.—If there is one fact to be gathered from this discussion, it is, I think, how remarkably little reliable knowledge we have about the subject on which we are talking. It seems to me that that is a lesson really of the greatest importance; because I cannot but fancy that a great part of the almost inextricable confusion that surrounds the whole matter has arisen from the non-recognition of that fact; from the circumstance that systematically writers upon the subject have thought it necessary to give a complete and fully rounded history of puerperal fever without recognising, as I hope they will do after this discussion, that we are only at the threshold of the inquiry, and that we have to build up all our knowledge by unprejudiced and patient clinical investigation. After saying this, I shall not err in the same way by talking too dogmatically in the few observations I have to make, and I shall confine myself, as much as possible, to one of the questions only which Mr. Wells has brought before us, and which has been talked of a great deal to-night—that is, the relation of the specific zymotic diseases to the so-called puerperal fever—a relation which, to my mind, is, in spite of what has been said about it, one of the most obscure subjects and most difficult to reconcile with theory (and which cannot be got rid of by simply denying it) of any question connected with puerperal fever. In the first place, my belief about puerperal fever is very much what I understand to be that of several other speakers. I do not believe that there is any specific condition justifying the

name of puerperal fever; nor do I believe that there is any special miasm arising from the puerperal patient capable of being conveyed to another patient; nor do I think that there is any evidence whatever to show that there has ever been an epidemic of puerperal fever in the strict sense of the word, although we all know how fatally endemic it has been in our large lying-in hospitals. In the second place, I believe that the theory which considers the so-called puerperal fever to be practically the same disease as surgical septicæmia or pyæmia, or whatever we choose to call it, is the one which is most consonant with the facts of the case; that it arises from the contact of septic matter, with lesions of continuity in the generative track, such as exist in every parturient woman; that there are channels of diffusion through the lymphatics, or possibly the veins; and that there are, just as there are in surgical pyæmia, secondary general and local results of great consequence, rendered in the puerperal patient particularly intense and virulent, on account of the peculiar condition existing after delivery, which was so graphically described at the first meeting by Dr. Richardson. But, sir, the moment that that theory is stated, a great number of difficulties and objections naturally present themselves, and among these I know none more difficult to resolve than that connected with zymotic disease. It has long been a recognised axiom amongst British obstetricians of the highest eminence, and I think I may venture to include Dr. Farre amongst the number after what has fallen from him to-day, that the puerperal patient to whom the poison of some specific zymotic disease, such as scarlet fever, is brought, may be attacked with an intense form of disease, which does not show the specific characters of the disease that produced the contagion, and which is practically undistinguishable from ordinary puerperal fever. I know that this view has been strongly controverted. It has been controverted by Dr. Savage, in his somewhat caustic remarks. It was controverted at the last meeting by my friend Dr. Brunton, who brought forward many practical facts of great value, as all

the facts that he brings before us are; but I think that Dr. Brunton fell into a common error — he argued from negative observations. I believe that no number of negative observations can outweigh even one single positive fact carefully observed. Dr. Brunton's argument reminds me of those fortunate gentlemen that I occasionally hear of from patients, who tell me, "Dr. so-and-so has attended a thousand, or fifteen hundred, or two thousand, cases of labour, and has never lost a patient." Now, I do not doubt the fact, and I congratulate the gentleman on his good fortune; but I must say that that fact does not to my mind vitiate the returns of the Registrar General, or make me disbelieve in the puerperal mortality throughout the whole of Great Britain and Ireland. So I think the facts in regard to these zymotic diseases are really beyond question. There is no one of the diseases about which strong evidence could not be brought forward. With regard to erysipelas, the fact is scarcely questioned at all. We all know how erysipelas is interchangeable in lying-in hospitals with puerperal fever. We had, as you, sir, will remember, frequent opportunities of observing that at King's College Hospital, at the time that we had a lying-in ward there. Then, with regard to scarlet fever, about which Dr. Savage has particularly spoken, I need only refer to Dr. Braxton Hicks' most valuable paper. There are, in that paper, no less than seventeen examples, if I remember rightly, of women who had a disease which presented no symptoms of scarlet fever, and which was clearly due to the contagium of scarlet fever. We all know that there is not, perhaps, in the room, a more careful observer than Dr. Hicks; and any man who can read that paper without a preconceived judgment, and who does not agree with it, has a mind which I believe to be not open to conviction. Then there is diphtheria; we do not see so many cases of diphtheria, so that we cannot judge of it; but I may remind you that the most brand-new theory of puerperal fever, that of Dr. Martin, is that puerperal fever and diphtheria are the same things.

I saw, within the last few months, a case remarkably illustrative of the influence of diphtheria in producing a disease which I was unable to distinguish from puerperal septicæmia. A newly married couple at Notting Hill had gone into a new house shortly before the confinement of the lady. The house was in a most unhygienic condition; an untrapped drain opened into the bedroom; an untrapped pipe from the drain, and a gully under the window, gave off offensive smells. The patient had an attack of intense septicæmia, from which she barely recovered with her life; and the husband, at the same time, was laid up with a well-marked acute attack of diphtheria, the wife presenting no symptoms of diphtheria. Who could rationally disbelieve that those two diseases were produced by the same septic poison? I do not think there could be any doubt about it. The arguments on the other hand are unquestionably of considerable force. There is one that Dr. Savage dwelt upon, viz. that patients have these diseases in the puerperal state typically developed and running a favorable course. I am well aware of that. There are many examples of the kind on record; and that is just one of the great puzzles which I hope time will explain to us, but which we cannot explain satisfactorily now. My own idea about it is—and I merely suggest it as an idea, which further clinical investigation of the matter may prove to be correct or not—that in the one class of cases possibly the contagious poison was brought immediately into contact with solutions of continuity in the generative track, and therefore produced an intense form of septicæmia; and in the other the poison may have been absorbed by the more ordinary channels. I think that is a sufficiently reasonable hypothesis, but one which, of course, we cannot prove. Then there is another argument, also of some consequence; viz. that if puerperal fever be pyæmia and if the poison of zymotic diseases can produce a condition which is not to be distinguished from pyæmia under certain circumstances, should it not be the case that, after surgical operations, these zymotic poisons should act in

the same way? The whole subject of surgical pyæmia, especially of its contagious qualities, has been so little worked at, that I do not think any surgeon would be justified in saying that zymotic poisons would not act in that way. I should like to know what Mr. Wells would say upon that point; and I hope that, when he comes to his reply, he will be able to spare a word or two with regard to it. But, knowing as I do the great care that Mr. Wells pays to his ovariotomy cases, I should be surprised to learn that he would be happy if he heard that some one was assisting him in ovariotomy who had just been handling either a scarlet fever or a diphtheritic case. I have but a minute or two to refer to that burning question, the conduct of the practitioner in cases of this kind, which is one of the most important connected with the subject. It is practically impossible, whatever we may think, to give up attending together zymotic disease and fever; nor, indeed, do I think that it is always necessary. I am quite at one with what Dr. Matthews Duncan has said in his letter that has been read here; that, if proper antiseptic precautions be taken, any risk from this source might be eliminated, whether we use iodine, or any other disinfectants that we like. I have no doubt that danger from this source might be obviated; but at the same time I think it quite consistent with common good sense that, instead of blindly shutting our eyes to facts, however unpalatable these facts may be—and no one considers them more unpalatable than I do—we should look them fully in the face. I believe the risk is not from the facts themselves, so much as from ignoring them and refusing to take the proper precautions which these facts would naturally suggest.

Dr. Tilt.—I am in the unfortunate position of a man who, having put down a few notes, finds himself forestalled by the observations of those who have spoken before him. I rise also under a feeling of some diffidence, inasmuch as I am afraid that I come within the

malediction of the last speaker as a man not open to conviction. On the first night of the discussion Dr. Braxton Hicks attributed puerperal fever to zymotic influence in three fourths of his cases. I was most interested with his paper; it certainly is a very valuable one, but open to the objection that he has put down as cases of puerperal fever, due to scarlet fever, "all those cases that he could trace as having been brought anyhow into connection with scarlet fever;" and whereas the paper contains twenty cases in which puerperal fever was said to have been caused by scarlet fever because there was a rash, which we should all admit, there were fifteen cases in which there was no rash; that seems to me a very large proportion of exceptional cases, and I should say that in all probability many of those cases were cases of puerperal fever arising from other causes, and not clearly from zymotic influence. If the zymotic influence is to have ascribed to it such an extraordinary effect in the production of puerperal fever, scarcely a puerperal woman could escape in the densely populated parts of a metropolis like London, where zymotic influences, call them scarlet fever, measles, typhoid fever, or anything else, are crossing and recrossing themselves with a frequency with which we medical men are fully acquainted. I am not alone in opposing this exaggerated view of the power of zymotic influence in the production of puerperal fever, for Dr. Robertson, who is at the head of the Maternity Hospital of Glasgow, immediately after the discussion, wrote to say that he did not adopt Dr. Braxton Hicks's views. Several gentlemen who spoke at the last meeting likewise controverted this over-estimate of zymotic influence, and several general practitioners from the country protested against it; so that I think, allowing full power to zymotic influence in the aggravation or production of puerperal fever, the theory was pushed a great deal too far on the first night of this discussion. With regard to the germ theory of puerperal fever, I will say just two words. I think the Pathological Society has been kind

enough to settle the question for us up to the present time. With reference to bacteria as a *post-mortem* phenomenon, it is quite an exceptional case. While Heiberg, at Christiania, was relating in an interesting paper, that he had found an enormous quantity of bacteria in the lymphatics of those who died during an epidemic of puerperal fever, other practitioners in Paris were making microscopic examinations in various hospitals, and found no trace of bacteria in the lymphatics. It is one of those exceptional facts that you meet with in puerperal epidemics; sometimes one condition will be found and sometimes another; sometimes pus in the veins, sometimes pus in the lymphatics, sometimes a fœtid sanious fluid in the lymphatics similar to what is found in the wound; and I think if, in this discussion, too much importance has been attached to zymotic influence as a cause of puerperal fever, too little has been attached to what I should call the autogenesis of puerperal fever, until it was so strongly and ably advocated by Dr. Wynn Williams. I think that, if a woman be sometimes poisoned by others, she more frequently poisons herself, and I do not see how you could imagine a better arrangement for poisoning a human body than you find in the womb if once you admit that it contains a fœtid fluid. You have a large bag, a spongy bag, partly bruised, sometimes torn; you have lymphatics abundant, lymphatics enlarged by the gravid process; you have lymphatics whose duty it is to absorb more speedily then than at any other time; they are bent upon absorption; if you have a putrid fluid, I was going to say that it must be absorbed, and, if it be absorbed, it goes into the lymphatics, and if it go into the lymphatics, you must admit now that it goes into the peritoneum, because it has been shown that the sub-peritoneal lymphatics open into the peritoneum. Here, then, you have not only the lymphatics, but you have the peritoneum liable to be inflamed; and, being inflamed, and inflamed early, from the fourth or the sixth day, for that is what has been observed. Peritonitis has been grouped with cellulitis

and other things as sequelæ, but it ought to be considered as something more than that; or, if it be a sequelæ, it is exceedingly near the beginning of the disease, and is quite sufficient to produce fever. I have shown the ichor of the fœtid lochia passing from the lymphatics into the blood—blood which is in a peculiar state. This poison may pass into the lymphatics without inflaming them; it is the same with all poisons. There are cases in which a woman has died of puerperal fever, in whom nothing has been found in the lymphatics but a sanious brown fœtid fluid similar to that contained in the womb. In other cases they may be, of course, inflamed. With regard to zymotic influence, no practitioner can deny its importance, and it acts in this way. We know that all zymotic diseases—take scarlatina for instance—cause the secretions of the human body to tend towards decomposition. When a puerperal woman is submitted to the influence of scarlet fever, it promotes the decomposition of all the secretions; so if they be already rendered fœtid by a portion of placenta, or blood-clot, or membrane of the womb, it renders them doubly fœtid, it renders the poison more virulent, and that sufficiently explains the frequent fatal effect of zymotic influence on puerperal women. Now, for the practical applications of the theory that Dr. Wynn Williams and myself more particularly advocate, it seems to me that, talking of sporadic cases of puerperal fever, the prevention of puerperal fever will depend to a very great extent upon the amount of belief that a medical man may have in the autogenesis of puerperal fever. If medical men will be on the look-out for fœtid lochia, they will find them a great deal more frequently than they do. I believe that consulting men in this room will bear me out that they have not unfrequently been struck with the fact that some accoucheurs, and still more nurses, are not gifted with a keen sense of smell. When we are ushered into a room to consult about a puerperal case, we are often struck with the offensiveness of the atmosphere, which they have not

noticed. It therefore strikes me that, if anything smells foul, medical men should not trust to the testimony of the nurse, as we too often do, and as I myself have too often done; we should examine and ascertain with our own fingers whether the lochia be fœtid or not. Should they be so, I think we ought not to be contented with vaginal injections, but ought to resort to intrauterine injections. I was talking, a few weeks ago, to Professor Stoltz, formerly of Strasburg, and he told me that, for fourteen years, he had been in the habit of making intrauterine injections when the lochia were putrid, and with the best results. There is concurrent testimony in favour of their value, not only amongst ourselves in this country, but in foreign countries; and not only that, but there are many instances of the sudden utility of antiseptic intrauterine injections; I mean the rapid abatement of the worst symptoms of puerperal fever, when the womb was washed out twice a day with disinfecting fluid. I know that there are objections made to intrauterine injections. An interesting pamphlet has been published by Dr. Goodell, of the University of Pennsylvania, who states that these injections are unnecessary, because no fœtid lochia can occur, he says, if you let the patient use the usual utensil several times a day, that will be sufficient to prevent fœtid lochia. That is an assertion, which would require to be confirmed by our own experience. There are two more important objections; one is, that the nozzle of any instrument that we might use would be itself a medium of infection. Of course, if it were carelessly used and improperly cleaned, one could understand the objection; but we have powerful means of disinfection at our command, and ifthe nozzle were steeped in a strong disinfectant I cannot understand how that objection would hold ground. The assertion is contrary to our European experience. Then there is another objection; the fear that the fluid injected should pass through the Fallopian tubes into the peritoneum. I can only say that, during the last few years, strong alcoholic solutions of carbolic acid have been injected into the womb, soon after

parturition hundreds of times in the Paris hospitals, and I ascertained the other day from a friend of mine that they did not know of any case in which death has been caused by the passage of the fluid injected into the peritoneum.

Dr. Braxton Hicks.—I wish to say that my paper has been misinterpreted. My paper was a communication with regard to cases of puerperal disease, not specially stating any particular point, but taking all the surroundings of the patients, and, out of those eighty-nine cases that I mentioned, I found that nearly half were associated with scarlet fever; some of them undoubtedly were dependent on scarlatina; but erysipelas, diphtheria, mental emotions, were also associated. I stated that erysipelas in lying-in hospitals was most common, perhaps more common than scarlatina in private practice. I left it as a clinical point not yet cleared up to explain these facts. That scarlatina also is associated with the production of what we are accustomed to call puerperal fever I think there can be no doubt.

The discussion was again adjourned.

JULY 7th, 1875.

William Overend Priestley, M.D., F.R.C.P., President, in the Chair.

Present—66 Fellows and 19 visitors.

Books were presented by Dr. C. Belluzzi, Dr. H. F. Campbell, Mr. W. M. Culpeper, Mr. W. Ross Jordan, Mr. Harry Leach, Dr. Ed. Martin, and Dr. E. J. Tilt.

Dr. A. Jukes (Ontario) and Mr. R. J. Morton (Aylsham) were declared admitted as Fellows of the Society.

The following gentlemen were elected Fellows: John W. Mason, M.R.C.S.; John Powdrell, M.R.C.S.; Matthew Reid, L.R.C.P. Ed.; and W. K. Waller, M.R.C.S. (Calcutta).

Mr. Edward Chaffers (Keighley) and Dr. Amos Rogers were proposed for election.

Dr. Edis exhibited for Dr. James Stothard (of Hull) a case of twins in which one died at an early period of pregnancy. Birth of the two at full period.

On February 12th, 1875, at 9 p.m. (Dr. Stothard states), I was called to attend Mrs. Jane D—, æt. 26, of her second confinement, her first child being seven years old, I should mention, by a former husband.

Upon making inquiries as to the previous and present state of her health, the nurse told me the old story—that she had been "queerly handled," which handling I

found amounted to a sensation of heaviness at her stomach after partaking of food, and also a constant sleepy condition.

Upon making a vaginal examination I found the os uteri dilated to the size of a two-shilling piece, and the presentation of the head of the child and what I supposed to be the hand placed upon the head. Although I could not make out the bifurcation of the fingers, accounting for that by their being enclosed in the membranes, I concluded it must be a hand by the concavity as seen afterwards, as being formed by the convexity of the child's head, and assured myself it was not a foot by ascertaining its thinness. However, departing, I believe, from the regular routine of practice, I thought the better plan would be (the head descending on the os by each uterine contraction) to rupture the membranes and allow the head to take the precedence, in which I was deceived. As the supposed hand rapidly descended and was born one hour after my entry in the room, I examined the structure unobserved by the attendants and found it to be a flattened fœtus; from its appearance I thought its uterine age to be four months, and enclosed it in a napkin, placing it secretly in a corner of the room until I had completed the labour, and had an opportunity for further investigation.

The uterus contracted regularly, and a living female child followed in the course of the fœtus one hour after, measuring in length 18½ inches and weighing 6½ lbs.

The fœtus had the colour of pipeclay, was flattened, and of the consistence of wet wash-leather, emitting no effluvia or signs of decomposition. It measured in length 7½ inches and weighed 14 drams, the weight of the combined placenta being 1 pound. The broadest diameter of the fœtal placenta measures 5 inches and the smaller 3 inches; the broad diameter of the child's placenta 7 inches and the smaller 6 inches. The funis of fœtus, cord-like and shrivelled, measures 14 inches, that of the child 15 inches. I have shown the specimen to Mr. Plaxton, pathologist to the hospital, who finds that the

fœtal placenta is indurated, and agrees with me that the fœtus has died at an early period and remained in utero until the time had arrived for expulsion of the living one. He had kindly drawn my attention to two extracts from Schröder's 'Midwifery,' pp. 34 and 158, bearing on this subject.

In conclusion, I may draw your attention to a rude sketch of the fœtus.

Dr. Newman exhibited a uterus and said:

It would only be wearisome to the Fellows present to enter into any long history of the patient from whom this specimen was taken. It will suffice to state that the operation of Cæsarean section was performed on July 23rd, 1866, in consequence of extensive disease of the os and cervix uteri, believed by those present to be true epithelial disease, and that from this operation the patient recovered.

In 1871 she became pregnant, and on January 11th, 1872, I delivered her with the long forceps of a child at full term.

The full history of both these occurrences can be found in the 'Transactions' of this Society, vols. viii and xiv.

In the early part of 1873 she came to see me, reported herself to be in very good health, and looked strong and well. "She wears an elastic bandage, which with a central pad keeps the abdomen quite comfortable. At centre of the scar and in the abdominal wall there is a thinned portion of integument, giving one the feeling of an ordinary reducible abdominal hernia of small size." I did not see her again before her last illness.

August 24th, 1874.—Mr. Row, of Market Overton, kindly wrote to me to say that the integument of the abdominal wall had suddenly given way, and that a large coil of intestine had protruded through the opening. He had enlarged the opening and returned the intestine.

25th.—Her husband brought a further report:—he told me that for the past three months, or even more, she had

had pain and inconvenience about the scar of the operation-wound; that the skin became a little rubbed and had never been well since, and that a sort of boil had formed which had burst; that there had been more or less of discharge since the boil formed, never in large quantity. She had never named this to any one. She was not making any effort when the protrusion happened.

26th.—Reported not to be so well; pain and sickness have come on.

27th, 8 a.m.—I saw her in consultation with Mr. Row. Pulse 120, wiry; face flushed; breathing thoracic; thirsty; a good deal of abdominal pain, relieved in measure by opiates. Had been very sick through the night, some stercoraceous material vomited yesterday. I made the following note of the local conditions.

The upper and lower ends of the scar are firm and sound; the middle is occupied by a depressed hollow, the integument shelving down at the sides to the centre, while at the bottom appears a portion of intestine which is not tense or strangulated in any way. The finger passed down goes first into a cavity which lies apparently between the layers of the abdominal muscles, and the boundaries of which cannot be defined, probably the sac of an abscess; deeper than this cavity there is a sharp, well-defined opening through the transversalis fascia and peritoneum, and through this opening the finger passes readily into the cavity of the abdomen. The abdomen is uniformly resonant, not specially tense. There is free pus discharge from the cavity of the abscess. I was not able again to see her.

29th, 12.30 p.m.—She died.

31st, 8 a.m.—I went over, not having heard the exact time of the death, and I was only able to make a very imperfect post-mortem examination. Very marked decomposition had set in. I opened the abdomen to the right of the mid-line. The portion of intestine which had protruded could be traced; a knuckle about three inches in length of the ileum, darker in colour than the rest of the

bowel. Very little trace of peritonitis, and that only near the wound and downwards towards the pelvis; none in the upper portion of the abdominal cavity. The sac of the abscess could be traced from the middle of the scar, dissecting out for some distance on each side of mid-line the abdominal muscles, and lying mainly between the under surface of rectus and the transversalis fascia; it extended behind the pubes and thence to the left side of the true pelvis, existing there as a well-defined elastic swelling. The peritoneal covering within the pelvis had not given way prior to my manipulations. I removed the uterus and ovaries. The latter were to all appearance perfectly healthy, the uterus was reserved for further examination, and is now before you. There was no adhesion whatever of either uterus or ovaries to any portion of the abdominal contents.

The PRESIDENT observed that it was in reference to this case Dr. Farre had desired to show one of the casts present this evening.

Dr. PLAYFAIR inquired if anything had been discovered in the condition of the cervix which would explain the difficulty for which Cæsarean section had been resorted to.

Dr. NEWMAN replied that he had only made a slight examination of the uterus. When pregnancy occurred in 1871 there were distinct traces of scar tissue, the cervix being deeply indented and changed in structure.

The specimen to which the above remarks apply has been deposited in the museum of St. Thomas's Hospital.

Dr. BARNES exhibited a dermoid cyst containing hair and several well-developed teeth, and a firm jaw-shaped plate of osseous substance. The cyst had been removed by ovariotomy by Mr. Rouse, at St. George's Hospital. It was filled with pus at the time of the operation. There was intense peritonitis, suggesting the idea that inflammatory action had extended from the cyst to the contiguous parts. There had been severe vomiting the week previous to the operation, attributed to a subcutaneous injection of morphia, and on which account the

operation had been deferred. The patient died on the second day.

Dr. Barnes also exhibited a large conglomerate of colloid tumours, weighing 28 lbs., which had grown from the omentum, being connected by a few large vessels only with that structure. The mass had been removed by gastrotomy, by Dr. Barnes, twenty-five days previously, and the patient was doing well. The vessels had been tied by silk ligatures which had been left in the abdomen. Many inflammatory adhesions of the tumour to omentum, intestine, and parietal peritoneum had been divided by clamp and actual cautery, copper and galvanic, and several vessels had also been ligatured. The tumour had been examined by Mr. Haward, who gave it as his opinion that it was malignant. [In December, 1875, the patient continued well.]

Dr. Barnes also exhibited a uterus from a patient æt. 45, married. She was admitted into St. George's Hospital on May 8th with an abscess under left arm, which had burst before admission. Had been subject to eczema for three years.

June 19th.—Was delivered of a female child; about seven months. Child died next day. Eczema had been getting worse up till now.

24th.—Eczema still getting worse.

28th.—Left thigh became slightly swollen and tender.

30th.—Left leg began to swell rapidly.

July 1st.—Died.

There was no peritonitis; there was diffuse cellulitis of left thigh; left femoral vein full of coagulum; internal surface of uterus covered with dark decomposing mucous débris.

Dr. Routh exhibited a specimen of extra-uterine fibroid removed by gastrotomy weighing 17½ lbs. His object was merely to show the tumour in its recent form. On a

future occasion he would give full particulars both as to the history and treatment.

The adjourned discussion on Puerperal Fever was then resumed.

The PRESIDENT.—We now proceed to the discussion on puerperal fever, which has been adjourned on two or three occasions. I may remind the Fellows and visitors of the rule which has been laid down, and which has been pretty closely followed, that each speaker should occupy not more than fifteen minutes. It is the general feeling that the discussion should close this evening, and, if necessary, the time can be prolonged for that purpose. If Mr. Spencer Wells be present, he will have an opportunity of replying. I may add that we are honoured to-night with the presence of two of our distinguished honorary Fellows—Dr. Fordyce Barker (who has come from America especially to take part in the discussion) and Dr. Charles West. Dr. Fordyce Barker has explained to me that he has much to say on this subject which may be at variance with what has been said by previous speakers, and he fears that he will scarcely be able to conclude within the prescribed limits; he, therefore, throws himself on the indulgence of the Society if he transgresses. Before I call upon Dr. Snow Beck to continue the discussion, I will ask the Secretary to read a short communication from a foreign Fellow—Dr. Cordès, of Geneva.

"In the latter part of 1865 the five children of Mrs. F— (one excepted) were taken with scarlatina. None died. In our country it is not so dangerous as it is in England. Mrs F—, being pregnant, but quite healthy and very strong, took care of them, and did not look fatigued by it, nor had she the slightest symptom of the disease.

"On January 1st, 1866, being taken with labour-pain, but having lost no water, she came down to her mother, about two miles away from her other children, whom

she left in the country, wishing to be confined in the town, and to avoid the contagion for the one baby to come.

"On the 2nd she was actually confined with a large beautiful girl. After a few hours the scarlatina began to show itself in the throat of the baby. The physician would not believe it was scarlatina, because there was no case in the neighbourhood.

"On the 3rd there was a consultation with another physician, and both agreed that the baby was suffering from scarlatina, which could not come out, and the child died on the 4th of the same month. The eruption came out a few hours before death.

"Now, is it not a singular thing that the baby could have been infected, being in its mother's womb, without the mother being taken with any kind of eruption, or erysipelas, or puerperal fever, or anything? Everything happened for her in that case as usual, except that she had no baby to nurse."

The following communication was also read in the absence of the author:

Dr. C. Holman.—The opinions of busy country practitioners must always be taken with a reservation, because from the necessity of their work they have but little opportunity to take notes. From their bodily or mental fatigue they can but register now and then exceptional cases. The opinions of a provincial practitioner must, therefore, be weighed by his character for accuracy of observation, and for honesty of purpose in drawing his conclusions.

It is, doubtless, an easy riddance of the question of the existence of an entity called childbed fever to lump all cases not septicæmic or arising from the contagion of zymotic disease, under the head of pyæmia; but I think I have seen a febrile condition beginning too soon after delivery to be dependent upon milk-irritation or pyæmia, attended by rapid pulse, high temperature, and all the symptoms of fever, without any local mammary or uterine local manifestations to account for it, running on for nine

or ten days and then undergoing resolution. I have no evidence of the communicability of this affection. They have not been fatal, and I could not class them as cases of surgical fever; as at present advised, I cannot admit the general rise of temperature spoken of by Dr. Richardson in women recently delivered.

Of the causes of childbed fever, pyæmia may at once be admitted as one. The only wonder is that it does not occur more frequently; the more often one examines post-mortem, the parts concerned in labour (shortly after delivery), the more one is astonished at the comparative rarity with which pyæmic fever happens. The ecchymoses in the uterine tissues, in the vaginal walls, the fissures in the uterine cervix, all appear as fertile causes of pyæmic mischief.

As regards septicæmia there can be no two opinions; but it is not as stated, the only cause of childbed fever. A large percentage of cases depend upon self-poisoning from retention of clots, portions of placenta or membrane in utero, or from lochial retention, such cases arising especially when the perinæum has been ruptured, has been stitched up, and complete rest enjoined. I saw a case quite lately where this had occurred; a puddle of fetid matter lay behind the repaired perinæum; the pulse was 150, the temperature 105°. A free washing out with permanganate of potash placed this patient in comfort and safety in forty-eight hours.

Dr. Brunton's opinions drawn from negative evidence are most dangerous. A man may practise for a great number of years; he may attend many cases successfully, he may go from post-mortem rooms, from cases of scarlet fever, from erysipelas, from almost any disease, and attend labours; but be sure his Nemesis will find him out, and in a manner the most unpalatable.

The experience of the Guy's Lying-in Charity speaks strongly on the dangers of the post-mortem or dissecting room. The average of cases attended is certainly over 1800 per annum. Amongst them some years since it was

found that students employed in dissecting, and post-mortem clerks, had an undue number of fatal cases occurring in their midwifery patients. Every care was taken to exclude those occupied in the post-mortem or dissecting room, and the amount of disease in the lying-in patients at once diminished. A friend of mine worked without anxiety for fifteen years as a general practitioner, but in one year he and his assistant had seven cases of childbed fever dependent on a midwife assisting to nurse a case of erysipelas in an adjoining ward of the infirmary. She poisoned her midwifery patient; in turn the disease was communicated to the two medical men, who in their turn conveyed it to six other patients within and outside the building.

I know a practitioner who had attended 4000 cases of labour and had only lost two within the month after confinement, except from phthisis (the same man turned for contracted brim of the pelvis some fifteen years before the late Sir James Simpson brought forward his cases); but shortly after stating these facts he lost one, if not two, cases.

Another attends 1360 patients without a death, attending all forms of medical and surgical practice besides. Scarlet fever creeps in, and death stays his career of success. This leads me next to express the opinion that scarlet fever may produce the disease proper, with eruption, sore throat, &c., mild or severe, in the lying-in patients, or it may breed childbed fever without any trace of eruption or throat affection, or any symptoms leading at once to the diagnosis of the primary factor being scarlet fever. The history can alone guide.

Some years since I had the opportunity of closely going into four cases of childbed fever attended by two practitioners.

Case 1. — A is summoned to attend a lady pregnant six and a half months, prone to premature confinements. She had one month previously been nursing a child with scarlet fever. On the first symptoms of labour she got

into a full-length bath, changed all her linen, had entirely fresh clothing, and removed to the other wing of the house. She was confined without difficulty.

Practitioner A was called immediately to Case No. 2, and was detained about an hour. On the following day but one, when dressed to dine out, and having no clothing on in which he had seen either of the other patients, he was requested by a sick medical friend to see a case of impending labour. He examined the patient, found he was not required, and left the house in a few minutes.

Practitioner B attends this case No. 3, and next day attends a patient of his own, Case No. 4. All these patients had childbed fever; none of them any rash.

No. 1 had the slightest possible sore throat on the ninth day, when she died.

Thus, Case No. 1 poisoned practitioner A, he conveyed the disease to Case No. 2; he left it at his single examination of Case No. 3; this patient communicated disease to practitioner B, who in his turn took the complaint to Case No. 4. Both medical men were tubbing men, cleanly washing fellows. Every other source of possible contagion was sought for in each case, but without success. They both ceased midwifery practice for a fortnight, changed all their clothing, and had no recurrence of disease amongst their patients. These cases I carefully went into myself and can vouch for their authenticity. Again, a lady expecting her confinement, about twelve miles south of London, had a child convalescent from scarlet fever about a month. She is advised by her medical attendant to leave the house, a very large one, and lie-in elsewhere. An old medical friend pooh-poohs the idea, and tells her to go into another part of the mansion, taking all precautions as to clothing, nurses, &c. She dies of childbed fever without rash.

Such cases must be taken for what they are worth dependent upon the character of the narrator for accuracy of observation. I have been a little surprised to find in the course of the discussion such strong expression of

opinion that childbed fever can only proceed from such and such causes, and from those only. Lengthened practice leads me to be very humble in proferring my own experience, and still more careful in impugning the experience of others. Septicæmia is not the only cause of the disease under discussion.

With the greatest possible respect for Dr. Farre, I would suggest that puerperal fever and post-partum fever are two enemies both under the same cloak. The public will soon discover this, and be as much alarmed at post-partum as they now are at puerperal fever. Personally, I think the old-fashioned name of childbed fever is the more honest name to recognise our foe by.

The very large majority of the fellows of this Society are active practical obstetricians. It appears to me that we are not the most likely men to hit off the peculiar essence of the contagious element of childbed fever. It will remain for deeper, subtler physiologists and pathologists than most of us are to solve this most difficult problem. That it will be given to us to know in due time I have the most devout belief. That we should at once expunge childbed fever simple from our nomenclature requires consideration; that the various causes I have referred to can cause the disease I am certain; that the zymotic diseases may cause it without specific evidence of the peculiar disease giving rise to it I feel persuaded. I believe in its communicability where arising from these causes, and no class of practitioners ought to be so careful in midwifery as those engaged in consulting practice.

I can have no hesitation in saying that if a practitioner meets with more than two consecutive cases of this disease in his own practice, he ought to relinquish work for a time; but where a fair interval of successful cases occur between his unsuccessful cases, then I hold distinctly that no blame can be attached to the man who continues his practice. I write of what I have done, and so I would be done by.

Dr. Fordyce Barker.—I feel that I have no right to waste

any of your time in personal topics, except briefly to express my grateful appreciation of the honour that I have received at your hands, and to appeal to your courtesy to excuse such deficiencies as may be charitably ascribed to weakness of the vocal organs or to that embarrassment which a stranger must feel in addressing such a body of obstetrical men on such a topic as puerperal fever. I may be permitted to say that this discussion is now watched with the greatest interest by your brotherhood on the other side of the Atlantic; but, as yet, this discussion does not appear to have led to that happy result spoken of by the Psalmist, that "maketh men to be of one mind in a house."

I shall aim, in the remarks that I am about to make, to confine myself closely to a discussion of the questions proposed to the Society, and to state my opinions, and my reasons for holding such opinions, in the most terse, compact language compatible with clearness. If these opinions seem to be in antagonism with a great majority of those which have been expressed in this discussion, I ask that they may not be regarded as partisan in their tone, but as a presentation of views necessary for a full and comprehensive study of the subject. I ask for such arguments as may be urged, that candid analytical sifting of evidence which is requisite for sound decision as to what is the true answer to the several questions now before us. I concede to all, and I beg all to concede to me, that it is the truth and not forensic success which is sought for by us all who speak in this discussion.

Of the six questions proposed, the second and third only admit a direct categorical answer of yes or no, and this will be one or the other, depending upon the answer given to the first question. If the first be answered in the affirmative, the second must be answered in the negative, and the third in the affirmative, or the answers may be exactly the reverse.

I suppose the meaning of the first question is clearly defined by the second and third to be whether there is a

disease peculiar to women in childbirth, never met with under any other circumstances, as distinct as typhus or typhoid fever, scarlet fever, measles, or smallpox.

I cannot think that the proposer of these questions regards it as necessary that those who believe that puerperal fever is a *distinct essential* disease must, therefore, accept all the qualifying phrases of the first question. For example, "distinctly caused by a special morbid poison" is one of the qualifying phrases. Is this a necessary characteristic of a distinct disease? Has science yet determined the "special morbid poison which causes typhoid fever?" When a disease is induced by contagion or infection, then it is "distinctly caused by a special morbid poison." If the phrase had been "often caused by a special morbid poison," I should have accepted it as true of puerperal fever, as I believe it to be true of diphtheria and some other diseases. It is just this phrase which seems to have debarred Dr. Farre and Dr. Richardson at the last meeting from answering the first question in the affirmative. And here I wish to remark that it seems to me that the idea involved in this phrase has been a great barrier to a clear conception of the disease, and has greatly contributed to the obscurity and ambiguity of its discussion. It implies that puerperal fever cannot be properly ranked as a distinct disease unless its cause can be proved to be an unit, "*a special* morbid poison." But this test is not demanded for many other affections which the profession universally accept as distinct diseases. There are many such which originate from multiple causes, so far as science has yet been able to determine, of which I may instance typhoid fever, diphtheria, erysipelas, and rheumatism.

A distinct character of a disease is determined by the evidence derived from three sources: the causes, the clinical phenomena, and the pathological anatomy. Some diseases give us proof of their distinctness by evidence derived from all three of these sources, others from only two, others from one alone.

Directly in connection and associated with the idea implied in the phrase "distinctly caused by a special morbid poison" is another idea, which fallaciously colours and obscures much of the reasoning on puerperal fever, and that is, that identity of cause must be followed by identity of result. In other words, a given cause—a special morbid poison if you please—known to develope a special distinct disease, must always produce this particular disease. In a wealthy family belonging to my *clientèle*, a year ago, I saw a lady dangerously ill from pyæmia, in consultation with Dr. Sayre, who was attending her for a traumatic injury. By our suggestion the house was carefully examined by a plumber, but no defect was then discovered which could explain the source of the blood-poisoning. Very soon three members of the family where severely ill from a typho-malarial fever. As soon as the condition of the patients would permit their removal, I insisted that the walls of the room covering the plumbing should be torn down, when it was found that there was a defect in the leaden waste-pipes sufficient to permit the noxious gases to permeate the house, but not sufficient to cause an escape of fluids to stain the walls of the rooms. During past winter I attended a gentleman with severe typho-malarial fever. His residence was in a large house, constructed on the plan of the French apartments, which was entirely occupied by families of refinement and abundant means. During the time of convalescence of this gentleman diphtheria appeared in another family in this apartment house. I saw the first child attacked, in consultation with Dr. Morris. Subsequently the father and two other children, one of whom died, were severely ill from diphtheria. It is worthy of mention that, while these were ill, the wife and mother was removed into another house, and she was confined with her fourth child, under the care of Dr. I. O. Stone, one of our most prominent physicians, and her convalescence was not impeded by any puerperal disturbance from either diphtheria, typho-malarial, or puerperal fever. On exami-

nation it was found that the waste-pipes in this house were in precisely the same condition as that of the house that I have just spoken of.

Who can doubt that the pyæmia, the typho-malarial fever, and the diphtheria were all caused by the same "special morbid poison?" At the last meeting of this Society Dr. Playfair related some facts which recently occurred at Notting Hill, where puerperal septicæmia in a wife, "from which she barely recovered with her life," and diphtheria in the husband, "from which he nearly died," seem to have been due to the same causes which I have mentioned. I ask you to note the language used by Dr. Playfair, for I shall again have occasion to refer to this. He says, "Who could rationally disbelieve that those two diseases were produced by the same *septic* poison?" One he names, because of his theory of its cause, septicæmia; the other, produced by the same cause, he names, from its clinical phenomena, diphtheria.

The other qualifying phrase to which I will simply allude, but which time will not permit me to discuss in detail, is the following: "as definite in its progress and the local lesions associated with it" as certain specified diseases. This qualification involves two distinct and different points, the clinical history and the pathological anatomy. I will only say that in my judgment these several diseases mentioned differ in degree as to their definiteness in progress and the definiteness of their local lesions, and consequently puerperal fever cannot be compared or contrasted with them as a group in these particulars.

I will now give my reasons for believing that there is a distinct disease which may be properly called "puerperal fever." I think the definition given in the 'Nomenclature of Diseases' emanating from the College of Physicians is absolutely perfect: a "continued fever communicable by contagion occurring in connection with childbirth, and often associated with extensive local lesions, especially of the uterine system." It is a disease which presents a group of general symptoms, independent of local inflamma-

tions, resulting from the absorption of some poison into the system. It is needless for me to discuss this point here, because it is evident from all that has been said that "localism" or "Broussaisism" has no *status* in this Society. Those who would call this disease septicæmia or pyæmia are in agreement in this point with those who call it a fever.

We can arrive at truth in medical discussion only by using language accurately. There can be no accuracy of idea without accuracy of language. It is the established usage of standard authorities in medicine to designate as fever all those diseases resulting from the absorption of some poison, which produce certain morbid blood changes and give rise to certain general characteristic symptoms, unless the specific poison is known. Under the denomination of fever are properly and legitimately included not only those diseases which are severally called typhus, typhoid, relapsing, remittent, intermittent, and yellow fever, but all the infectious constitutional diseases which occur either epidemically or endemically.

When the specific poison is known which causes the morbid blood-changes and induces certain general characteristic symptoms, the disease is named from that poison, and so we have the accepted terms in medicine of uræmia, septicæmia, pyæmia, and others of like character. I will here only allude to the fact that none of this latter class are known to be contagious or to occur as an epidemic. The point we are trying to settle is not a question of name, except so far as this. I doubt not that we will all agree that the name given to a disease should be significant and appropriate.

The gist of the matter, stripped of its superfluous and obscuring elements, lies in the inquiry whether there is a disease which attacks puerperal women and only puerperal women.

A necessary preliminary is to ascertain from what sources we are to obtain our evidence by which we can settle the question whether there be a distinct disease

peculiar to women in childbirth. I think we will agree that this evidence must be derived either—1, from the causes of the disease; 2, from the clinical phenomena; or 3, from the pathological anatomy.

I do not think that we are able, at the present day, to derive much information from studying the causes of this disease in settling this question any more than we can in settling the question whether typhoid fever, relapsing fever, or yellow fever are distinct diseases. In a sanitary point of view I think a study of the causes of this disease to be of vast importance, and for this reason I esteem the paper which was read by Dr. Braxton Hicks before this Society in 1870 as a most valuable contribution.

If a puerperal woman, not protected by the immunity of a previous attack or by idiosyncrasy, be exposed to the poison of scarlet fever or of any other infective continued fever, I have no doubt that she will have scarlet fever, or whatever specific disease she may absorb the poison of. The puerperal state does not protect her from the influence of such poison. Dr. Hicks, as I understand him, does not claim that puerperal fever is only scarlet fever in the puerperal woman, but names this as one of the causes in the same category with erysipelas, diphtheria, mental emotions, &c.

I think the evidence is conclusive to the minds of a great majority of the profession, established by numerous incontestable facts—facts observed not only in sporadic cases, but in very many epidemics, which have occurred not in hospitals or cities alone, but in epidemics which have ravaged large tracts of country in sparsely settled rural districts, that the poison of erysipelas may cause in women after childbirth a distinct disease, which some of us choose to call puerperal fever; and that the poison of puerperal fever, if absorbed into the system of a man or a child will cause in that man or child erysipelas. But the clinical phenomena and the anatomical lesions which result from this common poison are so diverse as to warrant us in regarding them as two distinct diseases.

Of course it is quite needless for me to refer to the medical history of Great Britian for proofs of this assertion; but I may be permitted to refer to that of my own country for most striking corroborative evidence.

A reprint of Nunneley 'On Erysipelas' was published some thirty years since, with notes by Dr. John Bell, of Philadelphia, in which he gives most striking facts in regard to several epidemics in the United States, in which erysipelas and puerperal fever have appeared together.

I will also refer to a valuable monograph on 'Erysipelas and Childbed Fever,' by Dr. Thomas C. Minor, of Cincinnati, published during the last year, and which I had the pleasure of reading on my voyage over. The work is based on a most painstaking and laborious study of the vital statistics of the census of the United States for 1870, and of a puerperal fever epidemic which prevailed in South Western Ohio in the winter of 1872. The work is well worthy of persual by all who assume to influence medical opinion on the subject of puerperal fever. This careful study of these vital statistics by Dr. Minor does not appear to show any connection between the typhus fever, scarlet fever, septicæmia and pyæmia, with puerperal fever. Epidemic scarlatina was very seldom associated with an outbreak of epidemic puerperal fever; but epidemic erysipelas was "*invariably* associated with an outbreak of epidemic puerperal fever, or *vice versâ*."

I have but a single remark to add in regard to the question of causes of this disease. The argument of those who deny that puerperal fever is a distinct disease rests mainly on the ground that the disease so called is the result of septic poisoning. Septic poisoning is a term now used very frequently and somewhat vaguely; but even those who contend that there is, properly speaking, no distinct disease of puerperal fever do not claim that all the resulting diseases from septic poisoning is one and the same thing, and that this disease should be called septicæmia. Is there anything improbable or unphilosophical in the hypothesis that septic poison acting on a system in

a peculiar state such as never is found in the human system under any other conditions, the state which has been so graphically described in this discussion by Dr. Richardson and Dr. Farre, results in a distinct disease, which never is found except when the system is in this condition?

Has any proof been offered by any one, anywhere, that epidemic and endemic influences, nosocomial malaria, contagion or infection, does not develope this poison in such a system, which results in a distinct disease?

We must find the answer in a study of the clinical phenomena and the anatomical lesion of the disease. If septic poisoning never occur as an epidemic, I do not say endemic, among those who are suffering from traumatism, and if it never develope contagion in this class of subjects, and if septic poisoning in puerperal women does occur as an epidemic, and is contagious and infectious, are not these elements sufficient to warrant us in regarding the two diseases as quite distinct?

The advocates of the septicæmic theory of puerperal fever, both on the Continent and in this country, have seen that they must accept this issue, and consequently they are driven to deny that puerperal fever ever occurs as an epidemic, or that it is contagious or infectious. One of your speakers at the last meeting said: "I do not believe that there is any specific condition justifying the name of puerperal fever;" and he logically adds, "nor do I believe that there is any special miasm arising from the puerperal patient capable of being conveyed to another patient; nor do I think that there is any evidence whatever to show that there has been an epidemic of puerperal fever in the strict sense of that word."

The author of the Address in Obstetric Medicine before the British Medical Society last year said in that address: "I have not been able to find anything worthy of the name of evidence to prove its epidemic prevalence at any time or in any large district." The same author, in his work 'On the Mortality of Childbed and Maternity Hos-

pitals,' says: "I feel certain, and I believe I can prove, that an epidemic of puerperal fever never occurred." He also denies, with equal positiveness, that the disease is ever contagious.

Now if, during certain years or seasons, puerperal women in a given territorial district die of a disease, call it either puerperal fever, puerperal pyæmia, or puerperal septicæmia, in numbers fiftyfold or a hundredfold greater in proportion to the births than they have in preceding years, or than they do in following years, I think the profession generally would call this disease an epidemic. When the mortality from puerperal disease, call it puerperal fever or puerperal pyæmia, in that part of New York which is in the best sanitary condition of any part of the city, and in which are the residences of the most wealthy part of the population, in four months of the year 1873 is twenty times greater than it had been for twenty-five years before, and the percentage of deaths in proportion to births is more than double what it is in the parts of the city where the poor women are crowded in tenement houses, and quadruple in the proportion of deaths during the same period from the same cause in the lying-in hospitals, I think we are justified in saying that an epidemic exists in this part of this city. If in Cincinnati, Ohio, in 1873, the number of deaths from puerperal fever was 122, while the annual average of deaths from this cause for five previous years was $13\frac{3}{5}$, I think most men would say that the disease was epidemic that year. In view of all the similar facts abounding in medical literature, I will not borrow a phrase from one of the speakers at the last meeting, and say that those who deny that this disease ever occurs as an epidemic, "must have minds which I believe to be not open to conviction;" but I will say that such persons must attach a subtlety of meaning to the word epidemic not consonant with (the) common sense. Please to observe that I use the article, and say *the common sense*, as otherwise the remark might seem discourteous.

I will not detain you now by a discussion of the question

of contagion, for all have undoubtedly fixed opinions on this point. Those who regard puerperal fever as a distinct disease believe it to be "communicable by contagion;" while those who believe it to be only pyæmia or septicæmia in women after childbirth, do not consider the disease contagious.

Can a woman after childbirth be exposed to the danger of receiving the poison which produces typical septicæmia in larger doses than when she has retained within her uterus a portion of decomposed placenta? If puerperal fever be septicæmia, would not the disease under these circumstances appear in its most virulent forms? But we all know that this is far from the fact. One very important idea in this connection was first distinctly enunciated by Dr. Barnes in this Society some years ago; and that is septicæmia in puerperal women is not actively contagious. "When arising from decomposition of the placenta, it generally began and ended with the patient attacked." During this discussion, Dr. Braxton Hicks, has expressed a very similar sentiment; and I feel quite sure that all clinical observers will coincide with this statement.

I will submit for your examination the following propositions:

1. The clinical phenomena of puerperal fever are quite different from those which are met with in surgical septicæmia or pyæmia.

2. These affections do occur in puerperal women, and the result is a disease which does not constitute a continued fever "communicable by contagion."

3. When either of these affections complicate puerperal fever, they modify the clinical phenomena by symptoms which can be distinctly appreciated and described by any close observer.

I will not take up your time by giving the evidence on which these propositions are based, because I think that this has been done in a work 'On the Puerperal Diseases,' which it will be indelicate for me to refer to in more definite terms.

I will add only a few words in regard to the pathological anatomy of the disease now under discussion. On the first evening of the discussion Dr. Richardson argued that there were no local lesions "which would lead us in the dissecting-room in any case to say this was a case of puerperal fever as we should say this was a case of scarlet fever or of typhoid or of typhus fever." I think it would be impossible to prove by the anatomical lesions that scarlet fever or typhus fever, or relapsing fever, are distinct diseases. A child is put in bed at night apparently well. A few hours afterwards it is awakened by vomiting, it is very delirious, its pulse is very rapid, and the temperature is six or seven degrees higher than is normal, and the child dies in the early morning without the slightest eruption on the skin.

No anatomical lesions can be found in the dissecting-room to characterise the disease. But scarlet fever is epidemic in the neighbourhood, or another child in the same family is taken ill with well-marked scarlet fever within a few hours after the death; and any physician would feel warranted in registering the cause of death as scarlet fever. Such cases are not very rare, and, I dare say, have been met with by several gentlemen now present. Would the most able of the recent authorities on the continued fevers—would Sir William Jenner, or Murchison, or Hoffman, or Lébert—insist that no one would have a right to register a death as resulting from typhus fever or relapsing fever, unless certain characteristic anatomical lesions are found in the dissecting-room? All of these writers declare that these diseases have no pathognomonic lesions.

It was asserted by the author of the Address in Obstetric Medicine that "pathologists believe that they have torn to tatters the view" that the disease we are now discussing "is an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid."

When? how? where? Show us the proof. Is there evidence that such pathologists have studied the disease

anywhere except in hospitals and large cities, where the disease is very probably complicated with septicæmia or pyæmia?

Of what value would be the researches of the ablest pathologists of Germany or England on questions relating to the pathology of yellow fever, if such pathologists had no opportunity of studying this disease except in Germany and England? Have Spiegelberg, or Schroeder, or Schmedeberg, or Mayrhofer, or Orth, or Heiberg, or Olshausen, or Fehling, or Cohnstein, or Breisky, or Von Haselberg, ever studied puerperal fever as it is found in epidemics in rural districts, where pyæmia and septicæmia are rarely met with? German is a difficult language to acquire; and it gives an attractive appearance of learning to introduce such names, even if nothing be quoted from them. I am familiar with their writings, and would not undervalue the merit of their researches; but so far as the elucidation of the question now under discussion is concerned, I think it of equal importance that we should carefully study for the clinical phenomena of the disease your English classical writers—Hulme, Leake, Kirkland, Clarke, Gordon, Hey, Armstrong, and Robert Lee. The hint on this point thrown out by Dr. Farre at the last meeting seemed to me most appropriate and timely.

What progress is made in science? Now do "we free ourselves from error," or gain in scientific precision or accuracy of description in giving to this disease a new name—pyæmia, as Dr. Duncan would call it; while he confesses that it has no etymological signification in this disease, and, in fact, has no definite positive meaning.

What propriety is there in giving to an obstetrical disease a name significant and appropriate to a surgical disease, unless it can be demonstrated that the two diseases are identical in their clinical phenomena and their anatomical lesions? The burden of proof to show this identity belongs to those who would call the disease puerperal septicæmia. Some think that the negative has already been established.

I have neither the time nor the voice to discuss the other questions which have been proposed. With my warmest thanks for your courtesy in listening to me so patiently, allow me to close with the suggestion that it may be well for all of us who discuss puerperal fever to remember the exhortation of Oliver Cromwell, when he lost patience with a Scotch Assembly: "I beseech you, brethren, by the mercies of God, conceive it possible that you may be mistaken."

Dr. Charles West.—I came here to-night because I felt that honour implies duty; and having now for some years had the distinction of being one of your Honorary Fellows, I felt that, if on the occasion of so important discussion as this, I stayed away, it would seem either as if I cared nothing about the exercise of my profession, or about the furtherance of it; or as if some other motive—I know not what—had kept me away from your assembly. What I have to say I will say in as few words as possible.

And, first of all, I have but one criticism to offer, and that bears on one of the questions which have been suggested with reference to the share which certain microscopic organisms bear, or may be supposed to bear, in the production of the disease called puerperal fever. It appears to me—with all respect to the gentleman who in his able paper proposed that question—that it is somewhat out of time to raise it. We have not as yet a sufficient amount of knowledge on the subject to be able to apply it, or to draw from the observations that have already been made any correct and useful inference. No one can, I think, have read with care the very interesting discussion on the subject of the "Germ Theory of Disease" without being struck by the way in which men of the greatest talent and sincerest love of truth differ, not merely with reference to the conclusions that they draw, but also with reference to the very facts that they thought that they had observed. I think, if this matter is to be settled, it must be settled in the first place in the

case of some disease simpler in its nature than puerperal fever. I pass, then, from that subject in these few words, in which I would still wish to convey the very high respect I feel for a kind of knowledge which I can make no pretension to, and to assure you that what I have said means no disparagement to study carried on by means of the microscope, and no doubt as to the great results that hereafter we may arrive at from it; but I refer to the subject from the conviction that the question so raised is, in the case of puerperal fever, at least premature.

I now approach the other and more practical question. It may be allowed me, perhaps, to remind the Society of what, I doubt not, they have not forgotten: that some thirty years ago, a physician now taken from amongst us, but whose professorial chair it was my high honour for a short period to occupy, wrote a work on the very subject which we are now discussing; and I do not myself see that we have advanced far, if at all, beyond the conclusions which Dr. Ferguson laid down, that the phenomena of puerperal fever depend upon a vitiated state of the fluids, and that, in the case of a woman in the puerperal state, such vitiated state of the fluids is specially apt to arise. I do not think, therefore, that we can in any way assimilate the condition of what is called puerperal fever to typhus, typhoid, or scarlet fever. The disease wants those characteristics which more or less distinctly prevail in the case of specific fevers. It has not the same regularity of course, the same regularity of incubation, the same regularity of outbreak. It has not the same diagnostic marks which we find in the case of the specific fevers; such, for instance, as the pustules which appear, even though in small number, and characterise even the slightest case of smallpox. Nor is this all. But with reference to these fevers we know, further, that for the most part, at any rate, they produce disease like to themselves; and that, further, though with many exceptions, they have a tendency not to recur where once they have happened. Now, all these characteristics are absent in

the case of puerperal fever; while the very circumstance that a far greater proportion of cases of puerperal fever occur in primiparæ than in those who have given birth to many children shows that there is a marked connection between the liability to an outbreak of the disease and the difficulties attendant on the process of labour. If, further, we expose a man to the contagion of puerperal fever, he does not get any disease as the result of it. If we expose a woman in the puerperal state to the contagion of scarlet fever, measles, or some other disease, she may have that disease, even though its characteristics may be masked and altered in some degree by the puerperal state; but it does not necessarily follow that she will have that same disease. Cases have been related by Dr. Collins, of Dublin, with reference to epidemics of puerperal fever, where the introduction of a case of typhus fever into the hospital was followed on each occasion by an outbreak of puerperal fever.

So it would seem that a cause distinct from anything which we can identify as the special poison of puerperal fever may yet, by the influence which it exerts upon the woman in this peculiar condition, produce a disease not distinguishable by any of its phenomena from that assemblage of symptoms which we ordinarily call puerperal fever. It seems as if the puerperal state itself was the condition of the development of this set of symptoms. Moreover, we must remember that there is not any one single solitary cause to which we can refer the symptoms of puerperal fever. It occurs now from one cause and now from another. I remember at this moment the case of a lady in a state of perfect health, who was delivered at a time when puerperal fever was not prevalent. She was a person in that position of society in which she was shielded from every possible danger. Soon after her delivery she was exposed to a current of air, and caught cold. In a short time the symptoms of puerperal fever developed themselves, of which she died. There was a case in which there was no special morbid poison existing

—nothing but what could be derived from her own fluids, or brought about by some impression on the system. Moreover, we know that mental shock will have the effect of producing the symptoms of puerperal fever, leading to a fatal issue. I remember the case of a lady who progressed perfectly well after an easy labour for ten days; but, after some violent scene of domestic strife with her husband, she was seized almost immediately with symptoms of puerperal fever at a time when no such disease was prevalent, and of that disease she died.

I think, then, we are to look for the cause of puerperal fever rather to the conditions present after delivery than to any special poison. Poisons of various kinds may produce it. We know that, in Vienna, puerperal fever was exceedingly prevalent, owing, as was discovered, in a great measure to the fact that the students left the dissecting-room and went with their hands imperfectly clean to attend the women in their confinements; and when greater care was taken, when careful ablution and disinfectants were insisted upon, puerperal fever diminished in its frequency to a very great extent. We know that local violence may produce it: in short, we know that a great number of causes may give rise to it, and that the only one factor in the production of that class of symptoms consists in the special state of the woman after delivery. That state I need not recall. It is a state in which the fluids are laden with old, effete, and useless materials; laden also with materials out of which the old uterus is removed and a new one is to be built up. They are laden also with those elements out of which eventually the new secretion of milk is to be established. None of us except those few who, like Dr. Richardson, have given great attention to these inquiries, can describe at all wherein the alterations of the blood consist; yet, without indulging in any wild theory, we may say that there is a something. People used to call it a ferment. I do not care what it is called, providing we use the name not as defining the thing exactly, but as a

name only to which we attach no other meaning than that of a something by which we are to distinguish this or that from the other, while as yet we know not its real nature. For instance, in the time of the old alchemists, calomel and corrosive sublimate, the chemical composition of which was unknown, were respectively called *Draco magnus* and *Draco mitigatus;* and if we use names as the old alchemists used theirs, we shall be much more likely to keep from the error of hasty conclusions or rash generalisations, than if we use a name to which we tack a definite scientific meaning; while as yet we have no scientific grounds to rest that meaning upon.

These were the chief things which it occurred to me might perhaps be worth the saying. Take a woman in whom the changes attendant on the puerperal state are checked in any manner, whether by poison from without, by mental shock, by cold, or by injury to the regular performance of those functions that ought then to go on, and you have the condition thereby produced out of which the assemblage of symptoms that we call puerperal fever—and all the world is pretty well agreed to give it that name—arises. The only other word I would utter is to quote a saying of a great wise old physician, who said that the highest aim of our art must be the greatest possible generalisation of diseases and the greatest possible individualisation of our patients; so that we have in each case to look, in the prevention of puerperal fever, not simply to the removing of possibility of contagion, not simply most carefully watching the local condition of the uterus, and removing from its inner surface any possible decaying matter, not simply to the taking care that the air that surrounds the patient is good, but we are bound also to consider the patient's mental state, to regard minutely her individuality, and, if we wish to put a stop to the disease, to remove from around or from within our patient anything which can interrupt those important processes, the interruption of which gives rise to those symptoms, which we designate by the name of puerperal fever.

Dr. Snow Beck.—I never felt so strongly the feeling that I would have been glad to escape from the position in which I may have been placed as I do this evening, after hearing the very learned and eloquent addresses with which we have been favoured. But I feel emboldened by the sentiment with which Dr. Fordyce Barker concluded, that possibly we may yet all be mistaken, to state the conclusion to which I have been led, chiefly from my own observations, upon the nature of the diseases which at present engage our attention. These observations have led me to a conclusion directly opposed to the opinions expressed by the previous speakers; for I am unable to perceive that there is any special or specific disease connected with childbirth, or to which the term "puerperal fever" can be properly applied. I must be also bold enough to contend that the definition published by the Nomenclature Committee of the Royal College of Physicians cannot be maintained in practice, for I have never yet been able to make out "a continued fever, communicable by contagion, occurring in connection with childbirth, and often associated with extensive local lesions, especially of the uterine system," which is essentially connected or associated with the condition of the system after parturition, or, in other words, which is not met with at any other period of the woman's existence. With reference to this definition, Dr. Arthur Farre informed us that the word "communicable" was only intended to convey that it might be communicated, but that it might also have a spontaneous origin; and the word "often" was only to call attention to the frequency with which these complications arise. Dr. Farre added that the Nomenclature Committee were entirely at a loss where the term puerperal fever ought to come in the catalogue of diseases. It was not placed among the fevers at all, but amongst the general diseases, far away from the fevers, finding a place of refuge at last at the bottom of the list after pyæmia, erysipelas, &c. But why should the Nomenclature Committee be in any such difficulty, if

there really existed any continued fever, communicable by contagion, especially connected with childbirth? Whilst admitting this difficulty, however, Dr. Farre said he was not prepared to admit there was no such disease as puerperal fever. He should be sorry to shut out the idea as one that is impracticable and not to be entertained. If we are to look for a puerperal fever at all, one that can be properly so called, a disease which is *sui generis*, we shall probably find it in the interruption to those healthy processes going on in the lying-in woman: first, the milk-process, the milk-secretion, and that attendant disturbance of the constitution which we term milk fever; and, secondly, a much more important one—the change which goes on in the uterus, and which we all know as the involution process. Now, we know, continued Dr. Farre, that when this process is interrupted there is an arrest of all the eliminative action which that involution process implies; and we may fairly conclude that, in consequence of this arrest, there is some accumulation in the body of those effete matters which ought to be expelled from the system, and this, added to the already blood-dyscrasia, must very much aggravate the disease. But may it not be replied that this sounds very like admitting that, up to the present time, no such special or *sui generis* disease has been recognised? If such a disease be developed in the changes which take place after the birth of the child, why has it not been found and described during the years of diligent study which so many of our most able and acute physicians have devoted to the elucidation of these diseases? And why is it that we have got no farther than the unsatisfactory statement that, if we were to look for a puerperal fever at all, we may probably find it in some arrest of these healthy processes? What proof, it may be asked, is there that, in the serious and fatal diseases following childbirth, there is any interruption of these healthy processes which go on in the lying-in woman—any arrest of this assumed eliminative action? Supposing, however, for a moment that such an interruption or arrest

should take place, what proof is there that this arrestment would create in the system so serious a disease—a disease *sui generis*, and capable of being communicated by contagion? It must not be forgotten that the growth of the uterus is a healthy physiological process; that the tissues which are formed are perfectly healthy tissues. When and where have there been any facts observed which justify, or even countenance, the supposition that the retention in the system of any portion of a healthy tissue will generate such a disease, that the majority of the women in whom it becomes developed will die? I am not aware of any facts upon which the least reliance can be placed which justify the inference that such a disease-creating action is even probable; and I must add that, instead of speaking of it as being possible or probable that such a *sui generis* disease would be found, if looked for, would it not have been more satisfactory, and certainly more conclusive, if the symptoms which would indicate such a disease during life had been given; or the appearance on the dead body described, by which its previous existence might be affirmed? So far as my knowledge extends, no reliable facts have ever been recorded which show that any such *sui generis* disease has ever been observed in practice; nor can I think that anything is gained by substituting the term post-partum fevers for that of forms or varieties of puerperal fever; for these terms are evidently intended to denote the same class of morbid phenomena, whatever their nature may be. In further considering these questions, I must contend that the process of childbearing is a natural function in the female, and that Nature has provided her with all the powers requisite for the complete performance of this function as a part of her natural endowments. Every day's experience appears to exemplify this fact, not only in the human female, but also in the female of all other animals. Yet, on the other hand, no one doubts that the peculiarities of the system after parturition modify, to a certain extent, any disease which may have existed previous to the con-

finement, or which may have commenced subsequently. But I am not aware of any observations which support the notion that there is anything in these conditions which, in a healthy female, is the source or element of disease, or which in any way can alter, or has altered, the essential nature of any disease with which she may be attacked. All women, however, are not in health when they become pregnant, and consequently are not in health at the birth of the child; and many women are not endowed by Nature with the constitution and formation favorable to the propagation of their species. Women in either of these categories must necessarily suffer from different diseases, and from various causes, after parturition; but the fault is here with the individual, and not with the natural and physiological process of procreation. But if it be true, as I believe it is, that there is not any disease specially connected with childbirth, it must not be forgotten that the converse is equally true, that there is not any disease from which the lying-in woman is exempt; in other words, that the condition of the system induced by pregnancy, or that which follows after parturition, does not prevent the existence of any other disease which may attack the female at any other period of her existence. Hence we must be prepared to meet with, in the lying-in woman, any of the diseases which flesh is heir to, and the diseases so met with become naturally divided into two classes—one class arising from the general system, and being the result of changes and accidents, to which all women are liable at any period of their existence; the other class arising from the uterine system, and being the consequence of accidents incident to pregnancy or childbirth, yet not comprising any disease specially connected with either of these states. When speaking of these diseases, Dr. Farre observed that he had been in the habit of dividing what he termed post-partum fevers into three classes: first, those irritative fevers arising from some local irritation, and not implying blood-infection of any kind, pyrexial states of a fugitive and transient nature resulting from simple mammary

irritation, slight injuries to the soft parts, laceration, and the like; secondly, under the name of the milder forms of infective fevers would be classed all those in which the infection is not of a specified nature, in which the process does not undergo a period of evolution, a period of development, and in which the consequences follow no definite order; thirdly, the eruptive fevers, and those which depend upon a blood-infection, the poison following a specific course, having a regular period of incubation, and terminating in those several diseases, eruptive fevers and the like, which occur, of course, in the lying-in woman in common with others. And, in glancing at the different diseases met with after childbirth, it may be well to do so under these heads. First, then, it would appear evident that those pyrexial states of a fugitive or transient nature, which are met with in almost every woman who has been confined, cannot be classed as belonging to the same category as those serious and often fatal diseases which follow parturition or abortion. We might as well include all the diseases in the nomenclature of diseases, for every one of these might by possibility be met with in a woman recently confined. Yet it is singular how often these trifling and transient affections have been considered as the commencement or form of serious disease. Dr. R. Ferguson, in his admirable essay on 'Puerperal Fever,' wherein he correctly attributes the most serious diseases to a vitiation of the fluids, though the nature and source of this vitiation is not so clearly stated as perhaps it might have been, remarks (p. 101): That a dose of salts and senna so invariably brought on metro-peritonitis, that he forbore the use of this drastic irritant as a routine dose; also (p. 16), that during a reputed epidemic of the peritoneal form, after two doses of Dover's powder, of ten grains each, the patients did not require any other treatment. Secondly, it is well known that the lying-in woman is often exposed to, and not unfrequently attacked with, the eruptive fevers, or acute specific diseases. I believe that every one of these diseases has been met with at one time

or another; and we cannot deny the proofs that they have been communicated by the nurse in attendance, and possibly, though very rarely, by the medical attendant. We all know that these diseases exercise a great influence, though in different degrees, upon the pregnant uterus. The rules under which women are received into the different lying-in hospitals, when the pains of labour have commenced, and often during the night, give great facilities for the introduction of these diseases. Different instances are recorded where typhus and other fevers have been so introduced into lying-in hospitals, and have spread from patient to patient, constituting a so-called epidemic. On one of these occasions, it is recorded that, after the introduction of a case of typhus fever into the Dublin Lying-in Hospital, "puerperal fever appeared in forty-eight hours afterwards, although the hospital had been free from it for a year before." But the disease which thus became developed was no doubt typhus fever; for evidence is entirely wanting to show that any of these infective or acute specific diseases have ever been known to change their nature. Some of the symptoms usually characteristic of these diseases may have been masked, or possibly absent in some instances, but their specific nature has in no way been changed. I cannot place any confidence in the statement that all these infective fevers "are excessively prone, if brought near the lying-in woman, to originate puerperal fever;" or that, by some wonderful and potent influence in a woman recently confined, all these different specific diseases are transformed into one and the same disease. Dr. Braxton Hicks appears to draw some subtle, and to me incomprehensible, distinction between puerperal diseases and puerperal fevers, and has carried this transmutation of diseases even farther than those who have preceded him. He imagines that these diseases not only change their nature when they enter the system of a woman recently confined, but when they again leave her system and enter into that of another, to quote his own words, "it becomes recontroverted into the zymotic

form from whence it had sprung." The experience, however, which has been adduced by the different Fellows during this discussion, appears conclusive that these eruptive fevers do not become changed in any respect when developed in a woman recently confined; and I feel assured Dr. Arthur Farre correctly stated the fact when he said, "I do not consider these diseases as in any way taking any part in the puerperal fever." Nor is there any evidence, such as in a scientific point we have a right to require, that the same cause can produce directly different results in different individuals, such, for example, as that mentioned by Dr. Playfair, where the same cause, included under the very vague term of "unhygienic condition" of the house, is said to have produced diphtheria in the husband, and puerperal fever in the wife? I must admit to having a mind "not open to conviction" by such tales, where the effects supposed to have been produced are contrary to everything that has been observed in the course of nature or in disease, and when the statement rests only on the imagination of the narrator, unsupported by sufficient facts. Thirdly, all these diseases of the general system which have been adverted to are of minor importance, as only a small minority of them prove fatal. They evidently do not constitute that serious class of diseases which come on so insidiously, continue gradually from bad to worse, and so frequently end in death, that, as Dr. William Hunter said, "treat them in whatever way you will, at least three out of every four will die." These diseases which have invested the term puerperal fever with such fearful significance, arise from the uterine system, and are included in Dr. Farre's second class; though I feel assured he has erred in supposing they are "infective fevers," or can be termed "the milder forms of infective fevers." They really constitute the serious, and too often fatal, diseases which are met with after childbirth. Let me sketch a case as illustrative of the class of diseases referred to. A healthy woman after a natural confinement, with or without hæmorrhage,

remains very well for two or three days; then has a rigor, which is followed by a very frequent and feeble pulse, a feeling of great prostration and sinking, some pain across the lower part of the abdomen, with considerable tenderness when pressed; gradual swelling of the abdomen and tympanitic resonance; considerable thirst; some sickness; with loose offensive motions. Gradually the patient becomes weaker and weaker; some shortness of breath, with slight cough and expectoration, is noticed, and she sinks after two or it may be three weeks, without more marked symptoms. After death, the peritoneal cavity contains more or less turbid serum, some soft, friable lymph is deposited on the peritoneal surface, particularly in the pelvis, with more or less injection of the peritoneum or the omentum; the uterus is large; when cut into, the tissues are healthy; the inner surface covered by a muco-purulent, somewhat reddish secretion; more or less pus-like fluid is found in the uterine sinuses, which are pervious throughout to fluid injected into the pelvic veins. What is the origin and nature of an illness such as this? A great variety of names have been applied to this condition, according to the view which authors have taken of its nature. It has been called peritonitis, puerperal peritonitis, peritoneal inflammation, metroperitonitis, &c., under the belief that the effusion of turbid serum in the peritoneal cavity, the deposit of soft friable lymph on the surface, and some injection of or under the peritoneum, were unquestionable signs of previous inflammatory action. But these morbid signs differ much from the appearances of undoubted inflammation of the peritoneum, and are now recognised as exudations which result from the impregnation of the general circulation with some noxious fluid or material. It has been termed phlebitis, uterine phlebitis, suppurative phlebitis, in consequence of the pus-like fluid found in the uterine sinuses, being considered unquestionable evidence of the previous existence of inflammation in these canals. Dr. Robert Lee has recorded several similar cases under

the name of uterine phlebitis, and has described the pus-like fluid as suppuration resulting from previous inflammation. But it is extremely doubtful whether inflammation can ever take place in the uterine sinuses. These canals are only channels formed in the contractile tissue itself, and lined by a delicate and apparently non-vascular membrane. It cannot be supposed that inflammation could occur in this delicate membrane; and if it occurred in the adjoining tissue it would be inflammation of the contractile tissue of the uterus itself. There has been considerable difference of opinion expressed as to whether the pus-like fluid found in these canals was the result of inflammatory action, or had been taken up from without through the open orifices of these vessels at the inner surface of the uterus. But this question has been definitely decided by the injection of the perchloride of iron into the gravid uterus to arrest post-partum hæmorrhage. Where death has followed in these cases, the uterine sinuses have been found gorged with black grumous fluid containing an abundance of iron, and as this iron could not be formed by any secretion from these canals, it follows that it must have been taken up from the inner surface of the uterus, where it had been deposited after injection. I suspect a similar explanation will account for these supposed cases of lymphangitis; the pus-like fluid found in the lymphatics not being formed in consequence of inflammatory action in the delicate walls of these vessels, but the vessels becoming filled by the fluid being taken up from without into their canals. I have not, however, had an opportunity of satisfying myself upon this point from the careful examination of any case. Cases similar to the one sketched have also been described as puerperal fever, typhoid form of puerperal fever, low typhoid pneumonia, typhus, &c., when the symptoms presented during life have alone been taken into consideration. They have also been termed pyæmia, septicæmia, ichoræmia, &c., from the symptoms and course of the illness being evidently similar to those which follow the introduction of noxious

fluids into the general circulation. It is to be regretted that so much confusion has been introduced respecting the meaning of these terms; but, so far as the present subject is concerned, it does not appear to be of much consequence which term is employed. Mr. Spencer Wells, in the opening address, says: "Supposing that the blood in the uterine veins clots, softens, breaks up, is the seat of chemical and vital changes, is detained in or near the pelvis, or is carried away to distant parts, or alters the composition and properties of all the blood in the body; that we have purulent infection or pyæmia, putrid infection or septicæmia: can all this arise in a healthy woman placed in favorable conditions, if she be not exposed to some morbid poison?" Now, the first thing essential to such changes must be the presence of clots in the uterine veins or sinuses; and as these clots have never yet been shown to exist, it is difficult to see how they could be the cause of so much that is supposed to follow. I am aware much has been said respecting the formation of clots in the uterine sinuses after parturition, and that these clots mainly contribute in many cases in arresting any escape of blood; but I am also aware that the blood during hæmorrhage does not come from these sinuses, that it is not a venous hæmorrhage by retrogression, and that these clots, about which so much has been said and repeated, have never been shown to be present in the canals themselves. Much, I am also aware, has been echoed from one writer to another as to the influence of hæmorrhage in predisposing the system to those conditions known as pyæmia or septicæmia; but the only relation between uterine hæmorrhage and septicæmia is one of sequence of events. Want of contraction of the uterine tissues allows the blood to escape from the utero-placental arteries; the same want of contraction leaves the canals of the sinuses pervious, allows noxious fluids to be taken up through their open orifices, and to be conveyed into the general circulation. These facts appear to be the answer to Mr. Spencer Wells's question. Can all the consequences

which he has depicted arise in a healthy woman placed in favorable conditions, if she be not exposed to some morbid poison? There cannot be a doubt that they can arise, have arisen, and will again arise, under the conditions named. Indeed, it has always appeared to me one of the extraordinary circumstances in relation to this subject, that such questions should ever have been entertained; for every one knows that isolated cases of the most serious affection, ending in the death of the individual, every now and then occur in all parts of the country, as well as in large towns, with ladies in good health, placed in the most favorable conditions, and surrounded by every care and attention that art can devise. If the succession of these cases occurring year after year be not sufficient to answer this question, I know not what evidence can be required. The same succession of cases appears to be an undeniable answer to the supposed influence of a vitiated atmosphere of overcrowding in any institution, or the supposed benefits to be derived from cottage-hospital isolation, &c. All that is required for the production of these serious conditions of the system is a means by which these noxious fluids can be readily conveyed into the general circulation, and this means exists when the uterus remains imperfectly contracted, or afterwards becomes partially relaxed, so that the canals of the uterine sinuses continue pervious, and any fluids formed at the inner surface, or which may have been injected into the uterine cavity, may enter at the open orifices of these canals and be conveyed into the general circulation. This want of contraction or subsequent relaxation, which practically is the same thing, may occur in the most healthy individual, living in the most healthy situation, and separated from actual or supposed injurious influences. Whilst the simple means to avoid the whole of these evil consequences consists in procuring and maintaining such an amount of contraction as will render these canals impervious, and prevent the passage of any fluids along them. When the want of contraction or the sub-

sequent relaxation is considerable, a portion of the placenta is often retained or coagula form in the cavity of the body from the blood which has flowed from the pervious arteries. In these cases, the sinuses also remain patent; the usual noxious fluids, rendered more injurious by the decomposition of the retained placenta or coagula, are taken up in larger quantity, when the symptoms become more marked and the course more quickly fatal. Whilst dwelling upon this cause of that serious condition of the general system arising from the uterine organs after parturition or abortion, and which I am under the impression has been much overlooked, I do not wish to infer that it is the only cause, or that other causes may not induce similar results. Severe laceration of any of the parts concerned in childbirth, followed by inflammation, inflammation arising from long-continued pressure of the head forced into the pelvis, the formation of abscesses in the cellular tissue outside the vagina or in the pelvis, and other accidents, may any of them produce the condition of septicæmia. But, except in rare and exceptional cases, I cannot think that slight lacerations are followed by such serious consequences as have been attributed to them. Laceration of the perinæum may exist at the same time as want of contraction in the uterus, but it is the want of contraction in the uterus which admits the septicæmic condition of the system to be induced, not the laceration of the perinæum. Once the way is open to the system being impregnated with noxious fluids or materials, the effect which is produced greatly depends (1) upon the nature of the fluids introduced, some being followed by no perceptible influence, others by serious and fatal consequences; (2) upon the amount; as in the experiments upon animals, a small amount produced disturbances of the system from which the animal recovered, whilst a large amount always caused death; (3) the state of health of the individual at the time of impregnation, for what to one appears a poisonous dose to another only causes general disturbance; (4) to continual renewal of the in-

fecting matters; as Cruveilhier remarked, the chief cause of the fatal result was, that the purulent infection was incessantly renewed from the original source. Moreover, purulent infections have not the same decided course of action as the poisons, whatever these may be, which originate any of the infective fevers or acute specific diseases. And when we take into consideration the various factors of disease which have been glanced at, combining together in very varying proportions in women recently confined, it is not difficult to perceive that we may often meet with combinations which puzzle our powers of diagnosis, and render it impossible for us to determine the exact nature of the illness. But, because we are unable to unravel some of these puzzles, or to determine the precise nature of any complicated or obscure illness, is that any reason why we should rush into the regions of romance, or, by concocting some high-sounding word, imagine we have solved a substantial difficulty or have explained a pathological entity? The question of contagion has been made the standing point from which many have endeavoured to maintain the existence of a special and essential disease occurring in connection with childbirth; but it would be impossible to enter upon this question at present. Some of the acute specific diseases, after being conveyed into the lying-in hospitals, have spread from patient to patient, and have thus been mistaken for an apparent epidemic. But the reliable evidence is not sufficient to show that any other disease than these infective fevers or acute specific diseases has been propagated either epidemically or by contagion. Most, if not all, the recorded histories on this point appear to admit of another explanation, and many to have their foundation in the management of the woman during the period of parturition, or from the circumstance that some minute yet essential particulars in the history of each case have been overlooked in the course of the illness. To quote the words of Dr. Fleetwood Churchill: "I rather think these accounts prove too much; for *post hoc* is not always *propter hoc*." And, so far as bacteria

are concerned, these bodies, we are told, are found in the interior of the organs in every dead body, from whatever cause the death may have arisen, their development being only a question of time and temperature.

Dr. Greene.—Assuming that the object of the present discussion is to clear up what has hitherto been confused and doubtful in post-partum pathology and therapeutics by a comparison of the records of individual experience, and not to establish any particular theory, I trust I may not be thought too presumptuous for desiring to bring before you and the Society the result of some ten years' observations, extending over nearly 2000 cases, of which a proportion of about 10 per cent. suffered, in a greater or lesser degree, from one or other of the various affections to which parturient women are peculiarly liable, and which have so frequently been confounded together under the general name of puerperal fever—a disease of which I had for some time doubted the existence, but which I am now satisfied has yet to be differentiated as a malady *per se*. So far from post-partum febrile affections being identical, I have observed that they invariably assume one of three distinct forms, differing essentially in their origin, progress, duration, and gravity—in other words, the exciting cause may be local and transitory, local and persistent, or altogether extraneous. In the first two forms the disease may be, and often is, autogenetic; but in the third this is necessarily impossible. Firstly, very alarming symptoms may be excited by the presence of a decomposing clot in the uterus, or even in the vagina, or from an overloaded state of the bowels, but yield rapidly to an aperient or the use of the syringe; and I may be permitted to observe that it is unpardonable in an accoucheur who fails to have recourse to such simple remedial measures on, or even before, the first appearance of the enemy. Some time since, one of my patients on the fourth day after a comfortable delivery had rigors, and when I saw her, had a pinched suffering expression of

countenance, considerable abdominal tenderness, suppression of the lochia, recession of the mammary secretion, high temperature (104°), rapid pulse (130), and, in fact, all the symptoms of so-called puerperal fever; but she also, as I was informed by the nurse, had begun to suffer from diarrhœa, which, on investigation, turned out to be the ineffectual efforts of nature to relieve the loaded intestines. I at once washed out the vagina, as there was rather more odour present than was usual, with a diluted solution of chloride of lime, my favourite antiseptic, and ordered five grains of calomel. Next morning, though informed by the nurse that she was weak and sinking, I found my patient wonderfully improved in every respect, but very angry with me because the powder I had prescribed had, as she said, purged her instead of stopping her diarrhœa, which, however, had then ceased to be troublesome. I ordered nourishment and a due proportion of stimulants, and she made an excellent recovery. Whereas, had I acceded to her wishes, and checked instead of promoted the alvine discharge, she would in all probability have died, for I have, I believe, in one or two instances seen a patient lost, because the attendant had an unreasonable dread of cathartics. Secondly, a foul, ill-ventilated, crowded, or overheated room, by promoting putrefaction of the lochial discharges, will often give rise to septicæmia, and, unless the cause can be removed, to death, which the most earnest efforts on the part of the medical attendant will, in such a case, be utterly powerless to obviate. Or, hysteritis, rapidly involving the adjacent parts, may result from injury to the uterus by long-continued pressure of the fœtal head in tedious labours; by the unskilful or incautious use of instruments; or by the well-meant or frightfully injudicious attempts of the accoucheur to pass the swollen lip of the os uteri above the head of the child during the continuance of pain—a very common practice, and often, I am convinced, productive of extremely disastrous consequences. A primipara, aged thirty-three, who had

suffered a good deal throughout her pregnancy from sickness and general debility, was delivered naturally after a favorable labour of a healthy living child, and progressed fairly for the first couple of days. She was nursed by her mother-in-law, who persisted in keeping the room at stove-heat. On the third day the patient had rigors, and severe symptoms soon set in, but subsided on the arrival of a more intelligent nurse. Unfortunately this person was soon called away, the mother-in-law became once more in the ascendant, stove-heat was renewed, all my orders disregarded, and the patient died on the tenth day after her confinement with every symptom of blood-poisoning. I might multiply instances, but *cui bono? Ab uno disce omnes.* In every such case—that is, ending in the death of the patient—I have no doubt (and this constitutes the essential difference between what I may call the first and second forms of post-partum febrile affections) careful inquiry would elicit the fact that the woman's health had been for some time in a more or less unsatisfactory condition; and to that cause rather than to any accidental lesion an unfavorable ending of the case must be ascribed, just as we not unfrequently find in the other sex, where the health has not been all that could be desired, the most trivial injuries giving rise to septicæmia and death; otherwise, nine-tenths of all women must sink after their confinements. In the third form, if I may so term it, of post-partum fevers, which comprises at least six-eighths of all cases, the contagium is conveyed to the patient from without in the shape of the germs of one or other of the zymotic class of diseases by the medium of some one—doctor, nurse, or friend—recently in attendance upon a person suffering from the same complaint; and of this fact I have had positive proof, although the possibility of its occurrence has been denied, and it has been even attempted to be proved that because some women resist the contagion, none can suffer by it—a mode of reasoning, however, undeserving of further notice. Of all the

exanthemata, scarlatina, in my experience, most frequently attacks the parturient female, and though giving rise to very alarming symptoms, is happily rarely fatal. Some years ago, during the earlier part of my professional career, I was attending a lad with scarlatina anginosa of a severe type, and at the same time nearly all my puerperal cases exhibited febrile symptoms, and had more or less sore throat, although the characteristic rash did not in every instance make its appearance. Several of the children sickened as the mothers were recovering, and exhibited every feature or scarlet fever. Fortunately, none of them, parents or children, died; but I received a lesson I shall not soon forget. Since then, at different times, I have met with other similar cases, and have always been able to trace the contagion to its source. Post-partum smallpox I have met with in two instances, where the preliminary symptoms were identical with those usually ascribed to puerperal fever; in both of these the patients recovered. Of post-partum measles I have had no experience, but I have met with several cases of erysipelas occurring in recently confined women who all succumbed to the disease. In one of these cases I was able positively to trace the disorder to its source. The nurse had come straight from the bedside of a man suffering from phlegmonous erysipelas—of which he afterwards died—to attend upon my unfortunate patient, in the raw placental surface of whose womb the insidious disease found too ready a nidus, and thence spread rapidly to all the adjacent parts, terminating fatally on the tenth day after the poor woman's confinement. The infant also died of the same complaint, thus confirming the diagnosis in the mother's case. I have had one case of post-partum typhoid fever occurring soon after delivery in consequence of the patient inhaling the noxious emanations of a drain immediately under the window of the breakfast room in which she had been confined. After a hard struggle for her life, this woman recovered, but her infant died with every symptom of

typhoid fever. If it be asked why scarlatina, &c., assume so formidable a character in parturient women, I reply that in them the absorbents are in the highest state of activity, and the poison in consequence is conveyed with extreme rapidity to all parts of the system, and produces a correspondingly grave effect. With regard to the animalcula, which by some are supposed to play so important and destructive a part in septicæmia generally, and notably in the case of parturient women, and by others to be a delusion and a snare, I cannot boast of any great experience; but I can imagine that their presence within the body would have a tendency to cause increased irritation, just as maggots in an open wound would certainly retard, if not prevent, its closing. Hence the value of antiseptic lotions, especially the chlorides, which are fatal to bacteria and their congeners. To sum up, there is no such disorder as puerperal fever, and therefore the erasure of the term from the nomenclature of diseases is very urgently demanded as utterly misleading, and in its lieu there might be advantageously substituted the name of the complaint from which the patient had suffered, with the qualifying epithet "puerperal"—thus, "puerperal septicæmia," "puerperal scarlatina," erysipelas, &c.; and a little care in analysing the symptoms and the history of each case would almost always render the diagnosis absolutely certain.

Dr. Routh.—I am anxious to make a few remarks in this discussion, since in early life it occurred to me to have seen, perhaps, a greater number of cases than any person in this room, having been present during part of that terrible epidemic which took place in 1846, and part of 1847, at the Vienna Hospital. During the first of those years, 456 women died of the disease, and in the second 176. The greater number of those cases I saw, and therefore I had an opportunity of studying the disease very closely. On my return to England I published a paper—which is extant, and is probably known to many

members present; therefore I shall only refer to it—in which I showed (as was also proved by Dr. Semelweiss, to whom the main credit is due, because it was his discovery) that the disease was communicated directly by inoculation from the hands of the medical students, who came from the dead-house and from the coroner's court, and satisfying themselves with simply washing their hands in a little water, made examinations of women about to be delivered. One point in regard to the disease was clearly brought out—namely, that it was not contagious from one person to another. That fact I took a great deal of trouble to find out, and I think it was proved in the most satisfactory manner that the disease was not communicable from one person to another, except the discharges were in some manner communicated, or except in the case of examinations made by the students under the circumstances I have mentioned. Many of the nurses and midwives who attended those patients were married women, and many of them had children, but not one of those women caught the disease. In the division where the labours were attended by women, who had nothing to do with dead bodies, the disease did not exist; or if it existed at all, the cases were so few and far between that there was no belief that it could be produced in any way by the attendants. And after the plan had been adopted of compelling every student to wash his hands in a solution of chloride of lime before he attended a midwifery case, the mortality in the first division, where about 3500 women were annually confined, diminished to exactly the same figure as in the second division, where a similar number of women were attended by midwives only. Well, that disease I showed from the evidence was not contagious from one person to another. That is exactly what is true of surgical fever. We all know that very often we communicate a disease to a lying-in woman, if we come from a case with our own fingers contaminated by some discharge. The records of medicine abound in cases of that sort. The cases published by Dr. Storr some years ago in the 'Transactions

of the Provincial Medical Association' showed how often the disease was produced in that way. Therefore, I say, it is clear from the evidence at Vienna upon a large scale, and from what I have seen and read upon a smaller scale —in my own practice and that of others—that one variety of puerperal fever can be, and often is, communicated by direct inoculation of the practitioner. In the case of the fever in Vienna the children died generally a little before the mothers, and in all those cases it was found that peritonitis existed in the child. In Dr. Storr's cases it was found that many of the husbands of these women contracted peritonitis; and we have heard to-night of some cases in which the disease communicated was erysipelas. Therefore we must admit that under certain circumstances there is some contamination that takes place between parties coming in close contact, probably analogous to that which takes place with typhoid fever which may be said to be communicated by discharges or by the powerful odour emanating from the patient. But no man can read all the literature upon the subject, I think, without being convinced that there is another variety of puerperal fever which is eminently contagious. Whether it has been communicated through erysipelas direct, or from typhoid, peritonitis, or from any other contagious diseases, it is communicable from woman to woman, even from woman to man, back again, as erysipelas, peritonitis, &c., or analogous fever. There is, I think, a point in which we have not made a clear distinction. There is indubitably a peculiar point in the contagion of several different fevers, at which, when it attacks a lying-in woman, it will develope in her a particular disease—call it puerperal fever, or call it a modification of the disease which originally infected her—but which, applied to another woman, would extend, and thus produce an epidemic. If we look at the question on this broad basis, we must come to the conclusion that some cases are not contagious and some are, and that those that are contagious are communicated by infection from other cases.

Dr. Greene.—I may mention a fact which I omitted to state,—that a midwife, whose son was suffering from erysipelas, communicated puerperal fever or some febrile affection, which undoubtedly was erysipelas, to a number of her patients, many of whom died. That occurred recently.

Mr. Wallace.— I am pleased to hear what Dr. Routh has said about the contagiousness or non-contagiousness of some cases of puerperal fever. Up to last October I was in the happy position of having attended over two thousand cases without having lost a woman from any disease whatever—like some of those gentlemen who write to the papers and say they have attended so many cases without a single death, and therefore think they are never to have one. However, my turn came at last. My *locum tenens* attended a primipara on October 5th. The house was in a bad hygienic condition, the rooms being ill-ventilated and most offensive, and the drains smelling. The woman died after six days of what I shall call typhoid fever modified by the puerperal condition. The most singular part about the case was this. I should explain that I came back from the country the very night the woman was confined, and I attended her afterwards myself. During the first two days after my return I attended four women in their confinement, and they all did badly,—none of them died, but they were all more or less ill for some time, and two or three of them barely escaped with their lives,—so much so that I was compelled to give up midwifery practice for three weeks, when my trouble ceased. About two months afterwards a medical friend of mine attended a case for me, in which the woman died after five days of metro-peritonitis. While this woman was ill I attended several cases, and they none of them exhibited any bad symptoms whatever. I continued to attend midwifery cases, and all my cases did well; but after two months there was another case where the woman died. I may mention that in the second case the woman had a dead child, and there was decomposition of a portion of

the placenta. What I wish especially to say is, that what would be called the pyæmic cases were non-contagious. I may also state that during 1870 in my neighbourhood there was an epidemic of scarlet fever. I had many cases to attend, and I also had a good deal of midwifery, but none of the women were taken ill. On several occasions children with the rash out were in bed with the mothers when they were confined. The same has been the case with measles. During the epidemic of smallpox which occurred in my neighbourhood in the east end of London, in 1871, those puerperal women who took smallpox had smallpox simply, not at all modified by the puerperal condition.

Dr. Griffiths.—You will, I trust, sir, pardon the remarks I am about to read, since I am not accustomed to take part in the debates of the Society. Indeed, I would ask you to pardon me for at all addressing any words to such an assembly as this, composed as it is of the Gamaliels of the profession; but I consider it behoves every member to give his opinion and experience, that conclusions may be obtained from such data. In the discussions which have preceded to-night's there seems to be some discrepancy as regards the relation of scarlet to the so-called puerperal fever, which with the greatest diffidence I submit may be explained in the following way: some gentlemen have seen and described true scarlet fever running the regular course, the subject of it happening, in the chapter of accidents, as it might be said, to be the lying-in woman, in whom that fever which we know by the name of the puerperal fever was not induced. Other gentlemen have seen, after labour, symptoms which they have considered scarlatina, but which really do not constitute that affection, being simply the outward and visible signs of that inward autogenetic poison, of which—as indeed the word "autogenetic" implies—the woman is herself the factor, and subsequently the recipient—recipient not alone, because the poison generated in utero lies in, or is received

by, the uterus, but because it is received into the lymphatics, the veins, the peritoneum, and into the body generally, as described by Dr. Tilt in his paper on 'Lymphangitis in Pelvic Pathology' in the sixteenth volume of our 'Transactions,' and alluded to by him in his recent speech. With the train of symptoms to which I refer, and which has been termed scarlet fever, who, practising obstetrics and is not acquainted, and has not met it in cases of what, for the sake of distinction, we must term puerperal fever? But, sir, is it scarlet fever at all—at least that form contracted from another affected with the ailment, and which, in the usually accepted meaning of the name, constitutes the true disease? The conditions alluded to, and which are identical with scarlatina, nay, more, are in some instances, perhaps, the same, are the result of pyrexial action, the indication of latent mischief, and have their origin not from scarlet fever contagium—that is, for a scarlatina-infected person, the woman never having been exposed to the scarlatina virus—but from the presence of some putrid or offending matter in utero or vagina; the sickness, the dreadful headache, the hot, pungent skin, the sore throat and rash, the congested eyeballs, the jaundice, the delirium, the oppressed heart, and breathing, and later, the diarrhœa. These are not scarlatina in the usually accepted sense of the word, but a group of symptoms pointing to a toxæmic condition of blood dependent not upon the entrance of a specific virus, but simply of the noxious elements resulting from putrescent animal matter—are, in fact, the sequelæ and manifestations of the blood-poisoning which occasions what is termed puerperal fever, and which, if not checked or corrected, would inevitably end fatally. In a scarlet fever epidemic, such happening to a lying-in patient would be set down to the scarlatina poison; but I have seen them where no scarlatina at all existed, and where no exposure to it had taken place. Those gentlemen who have seen scarlet fever attacking the lying-in, and running its usual course well defined without inducing any form of puerperal fever,

would do good by relating whether the lochia were offensive before the scarlatina set in, or at any time during its progress. If not before the invasion of the fever, then I say the case is one of true scarlet fever; but if the discharge become offensive only in the course of the scarlet fever, I would submit that the fever poison, perverting, as we know it does, the entire blood, has vitiated, and it may be even rendered putrescent, the outcome of the blood—I mean the lochia; the relation of the scarlet fever being to the offensive discharge as cause to effect, the reverse of that which obtained when the symptoms exist which I have demonstrated as being those of the so-called puerperal fever, and to which we might apply the term "bastard" or "simulating scarlet fever." Those gentlemen who hold that scarlatina is a great agent in producing puerperal fever, would do well to tell us whether the scarlet fever symptoms came on before labour. If not, on what day after delivery? They should also state whether the lochia were fetid. If they were, how many days previous to the scarlet fever symptoms had they been so? I am led to the conclusions I offer by a case of autogenetic poisoning at this moment under my care, in which most, if not indeed all, the symptoms of scarlatina obtained, but which recognising to be wholly the consequence of a morbid condition of the uterine system and its excretions —the lochia—I attacked by imitating the eliminative action of involution, grasping the soft, spongy, uncontracted uterus with my hands, compressing it and expelling some clots, washing out the putrid or putrescent discharges of the womb and vagina by means of Higginson's syringe, Condy's fluid and warm water being used every two hours, till the lochia became quite inodorous. Had I, however, seen the patient for the first time, when those apparently scarlatina symptems were well developed, I might have erred in supposing I had a case of scarlet fever to deal with, have treated it as such, and lost my patient; but, being alive to the puerperal facts to which I have drawn attention, I at once proceeded as I have

described, and the correctness of this diagnosis was evinced by the immediate improvement in my patient, commencing as it did from the very time I got rid of the offensive clots, and washed away the foul lochia. All the group of evils in this case might be termed scarlet fever, yet, all at once disappearing under the defouling treatment named, showed incontestably that the diagnosis was correct. Moreover, since her recovery, I have made inquiries, and find no exposure to scarlet fever nor any presence of it in the town. One difference I noticed; there was no desquamation of the skin, although I should not have been surprised had such taken place, so high did the temperature run. I admit, as Dr. Savage, I think, said, there may be cases of septicæmia running a rapidly fatal issue without any of the pyrexial disturbance I have named, or even any elevation of temperature from first to last; but that is where the patient is stricken down and overwhelmed by the dose of poison, or where the febrile symptoms occur in our absence, and hence may escape the observation of even the acutest. The same remarks will hold good of the rash as adduced by Dr. Braxton Hicks. I am led up to these conclusions because lately my mind has been exercised upon what I consider to be a fact—namely, that scarlet fever and typhoid may originate in the same way, from the same infecting source or poison; may be, in fact, the same poison developed in different persons or in the same persons at different times and under different circumstances, the poison at one time determining to the lymphatics and glandular systems, producing what we know as scarlet fever, at another time determining with greater virulence to the intestines, particularly Peyer's patches, as exists in typhoid fever. In this—although I had arrived at my goal without knowing the way to it had been trodden before me—I find I am borne out by the previous researches of Dr. John Harley, which he published in a pamphlet he has had the kindness to send me since I commenced my investigations; also by the researches of Dr. Tilt, made known to us in his paper on "Lymphangitis

in Pelvic Pathology ;" by Dr. Tilt's speech, by the speeches of those who have preceded him, and by my own at present somewhat crude experience, which I value very slightly indeed till I verify and check it by further careful inquiry. Bearing in mind, therefore, the life symptoms which exist in scarlet fever, puerperal fever, and typhoid, also the pathological conditions found after death, and how closely allied they are—nay, more, how much alike in a number of instances, we can see how readily the first—that is, scarlet fever—can run into the second, puerperal fever ; the second into the third ; how typhoid came to be supposed to originate puerperal fever (Sir Henry Marsh's opinion, as quoted by Dr. Squire) ; and how true so-called puerperal fever may clothe itself in the garments of scarlet fever, so that only with difficulty can it be distinguished.

Mr. Spencer Wells.—The chief difficulty I have in replying to-night in the few minutes that remain before the end of the discussion, consists principally in the great number of speeches that have been made, and the extremely numerous and important topics which have been suggested by different speakers. But I think I may congratulate the Society upon the very great importance of the discussion which has been carried on, and upon the very great ability which has been shown by a great number of gentlemen who have addressed us, and by none more so than by our friend from the other side of the Atlantic, to whom we have all listened with so much pleasure and instruction to-night. I will, with your permission, while what our friend has said is fresh in our minds, refer to two or three topics which he has raised, inasmuch as that will, perhaps, prevent the necessity of a good deal of repetition in discussing the first three questions which were placed before the Society in the little programme by which I endeavoured to give a direction to the thoughts of gentlemen who were going to speak. There was really nothing in those questions intended, in the way that Dr. Farre supposed, to lead to any very definite conclusion, or to any kind of sugges-

tion that I myself had formed any decided or positive opinion upon them. It was more with the hope that different gentlemen would take up the different questions according as their opportunities gave them means of information, and would bring before us the results of their own individual experience upon these different questions. Dr. Barker at once started the question by his statement that we must, as he says, from identity of cause presume identity of result; or, at any rate, that, if we have a specific morbid poison to deal with, that specific morbid poison should always produce the same results; or, as I think I understood him to say, that you might, from the same specific morbid poison, get very different results in different women under different conditions. For instance, given a morbid poison, and that morbid poison received by a healthy woman, it would produce in her a very different condition from what it would produce if received by a puerperal woman, and by the puerperal woman if she be in an unhealthy condition at the time. I do not know that I exactly interpreted his meaning; but, if I did, I should like to say that I would go somewhat beyond that, and assert that, if you have a specific morbid poison, it must, under all conditions, produce identity of results. I do not believe, if you inoculate smallpox in a patient, that you can produce a chancre; I do not believe, if you inoculate a chancre in a patient, you could produce hydrophobia. I believe you as certainly produce identity of result from identity of cause in the animal body as you do in the vegetable. If you sow a grain at the proper season in a fruitful soil, the seed will germinate; if you sow a grain of wheat, you do not get barley or oats. The seed germinates, and you get the same variety of vegetable produce. It is the same, I believe, with animal poisons. If you sow a seed of hydrophobia, or a seed of syphilis, or a seed of smallpox in a patient, you get a crop of smallpox, a crop of syphilis, a crop of vaccine, or a crop of erysipelas, according to the poison you have sown. It is the same with medicine. If you give a dose

of castor oil, you do not send the patient to sleep; if you give a dose of opium, you do not produce purging. All these poisons produce the same result. So with puerperal fever. If you suppose that in puerperal fever you have a specific disease different from any other fever, or from any other condition which we meet with in surgical or medical practice, you must show us some cause which will, under all conditions, produce that disease just as certainly as you produce smallpox by inoculating smallpox or scarlatina by exposing a patient to the poison of scarlatina. That carries me to the question which Dr. West started, as to what share bacteria, or some other form of vegetable or animal life in its lowest form, may have in producing these diseases, or carrying the poisons that produce them. I agree with Dr. West, that we are only on the threshold of this inquiry, and the question which I put before the Society was simply what relation have bacteria and allied organic forms to the pyæmic process in the puerperal state; in other words, to puerperal fever. I put the question in the hope that Dr. Burdon Sanderson or some other of the gentlemen who have lately made such important investigations on this subject, who have investigated the subject of splenic fever, sheep-pox, relapsing fever, diphtheria, and erysipelas, would have come here and given us the benefit of their experience. The great use of this discussion has been, that it has brought here not only the ordinary Fellows of the Society, but others from distant parts — a professor from Glasgow, whose candour and ability excited universal admiration amongst us, and a hope that some day he may occupy a still more important position than that which he now fills so creditably—some of our past presidents, and several of our honorary Fellows and gentlemen from the provinces practising obstetrics very largely. It has also brought one distinguished both as physician and physiologist, and another who is among us to-night who I hoped would have spoken, who has done more, perhaps, to prevent disease in this country than any other man—Dr. William Farr—who, by making known the

incidents of diseases, and tracing their causes, has done more, perhaps, to check preventible disease than any one else. I hope that at some future time he will favour the Society with some statistical account of the prevalence of this fever in different localities. We have been delighted by a promise from Dr. A. Farre of another paper on the subject, so that, at any rate, the discussion has not been fruitless in good to the Society, and I trust that it will be beneficial to the profession at large. We have been favoured by Dr. Farr with an account of a recent epidemic of puerperal fever in one district of South London. It is only for the week ending June 19th. In the printed returns of three deaths it was stated that the cause of death was given as peritonitis and fever, which were presumably cases of childbed fever. There were three cases in that week and one in the preceding week, which it was found, on investigation, were all attended by one medical man. Between the first case and the other three he attended two cases without a fatal result, so that the first case was probably the cause. It has not yet been quite ascertained how the first case began,—whether it was a case of ordinary peritonitis or not. The investigation is still being carried on. Scarlatina is still prevalent in the district; in the same week there were nine or ten deaths registered. An interesting question arises, and is being investigated—whether the two women who escaped from childbed fever between the first and third cases by this gentleman were protected by previous attacks of scarlatina. That is a most important matter; it is still being investigated, and I hope Dr. Farr will let us know the result. I have one or two letters here showing the very close connection of scarlatina with puerperal fever, which it will perhaps be interesting to the Society to read. One is from Dr. Jackson, of Notting Hill. He gives the particulars of a case that occurred in September, 1864. "The husband, about 30, had a severe attack of scarlet fever, from which he recovered. The wife, who had, some years previously, had the fever,

and who was in the end of her eighth month of pregnancy, waited upon and attended her husband day and night. In about fourteen days from the date of her husband's illness she began to feel ill, shivering and feverish, but had neither sore throat nor eruption. Labour suddenly and sharply set in. The child was born alive, well developed and strong, but covered with the distinct and characteristic scarlatina rash. From birth it was restless and uneasy, and died on the third day. The mother progressed most satisfactorily, and feverish symptoms abated. Milk and lochia were well established. Some slight apparently dyspeptic symptoms were complained of at the end of a fortnight, but these gradually disappeared. The cuticle on both hands desquamated. At the end of the fifth week she was quite well, and left town for the country." I have several other letters giving instances of the close connection which Dr. Barker has alluded to between erysipelas and puerperal fever. One is a letter from Mr. Freer, of Stourbridge. "Some years since I left home for a few days' holiday, leaving two patients expecting their confinements. The evening before I started, my father was thrown from his horse upon the forearm and right olecranon. The next morning the arm was red and swollen. Two days afterwards he attended both my patients, who had very natural labours, not requiring any assistance, and my father was not half an hour with either of them. Upon my return on the fifth day I found both my patients dying, and my father's arm so much inflamed, that I had to make incisions from the elbow to the wrist. Each case had rigors in twenty-four hours. Two years since I was engaged to attend the wife of a clergyman in her first confinement—a very fine healthy lady, aged 26. Upon entering the bedroom I found a nurse in attendance with an erysipelatous blush and swelling upon the side of the face. Upon inquiry she told me that, two days before, she had been in a Liverpool hospital to have the nasal duct opened. My patient was seized with rigors at the end of thirty hours,

and died of puerperal fever on the eighth day. The nurse died of rapid erysipelas of the head and neck on the twelfth day." I have also a letter from Mr. Pemberton, of Banbury, which I need not read through. It goes to show that, in a house where there is typhoid fever, a woman being confined may get puerperal fever; but then I do not think at all that it follows from that that the cause was the same. You may from drains get a variety of poisons, a variety of chemical and organic products thrown out, and one patient may get one and another another. You may get sulphuretted hydrogen, phosphuretted hydrogen, and a variety of chemical substances and vegetable or animal organisms of a low form, and one may affect one patient and another another. Showing how this bears upon surgical practice, and somewhat in answer to the question which Dr. Playfair started, I will read a letter from Dr. Keith, of Edinburgh, giving me an account of a most remarkable series of cases in which erysipelas with puerperal fever, and septicæmia after ovariotomy, seem to have been in close association. "On May 19th my friend who assists me at my operations became uneasy for the first time about a puerperal case. The patient died on the 26th. On the 24th he helped me at an ovariotomy, but did not see his puerperal patient till after the operation. Next morning my patient was in a typhoid state; she had bad bedsores, all sorts of eruptions, and superficial abscesses, but recovered after this pyæmic condition had lasted a month, and had no suppuration in the pelvis. On the 30th he assisted me at a tedious ovariotomy. The patient was quite well for six days; pulse and temperature were natural. She then suddenly shivered; the pulse rose to 170, and the temperature to 106 within a few hours, and she died five days after. On the same day (the 30th) he opened a small superficial abscess in a lady's neck. She went after this to the country; was quite well for five days, then shivered and died five or six days after of erysipelas of the head. On June 6th he attended a simple case of natural labour

in a healthy woman. She shivered the next day, and died on the 10th, of septicæmia. As he went home from this confinement he himself shivered, and was laid up with erysipelas of the face, from which he recovered. The medical man who attended the last puerperal case during the few days she lived examined for an instant a patient in the first stage of labour, four days after the death of the last case, but declined to attend her. This patient had metritis, and is now recovering, the veins of the left limb being all plugged up." I have seen this kind of thing myself in surgical practice, and it has led me to be extremely careful in getting each gentleman who attends the operations at the Samaritan Hospital to sign a paper before going into the room that he has not attended an infectious case for seven days, and has not been present at a *post-mortem* examination during that time. I believe that a precaution of this sort would really very much protect puerperal women from many of the infectious diseases to which they are subject, and which, I believe, are—as scarlet fever, or as measles, or as smallpox, or as erysipelas—confounded together under the name of puerperal fever. I hope the end of this discussion will be to give more accurate names to the different conditions which have been confounded together under that one name. I should like to say a single word as to the great question of what is called homicide by infection, and the attempt that has been made, I think unfortunately, to enlist the sympathies of this Society in the protection of careless and ignorant women, who deserve the punishment they have got. I think if a woman be cautioned by a medical man, who tells her that she has attended a woman in her confinement who has died of puerperal fever, and that, if she attend other women, she is liable to poison them, and, if she go on in defiance of this caution, to attend other women, and says, "Well, if this one die, I will give up," such a woman is not deserving of the sympathy or help of this Society. I think we should do good service if we rather stimulated the Fellows of the Society

and the midwives whom they employ not to carelessness, but to additional caution, teaching them not only to wash their hands, but to use disinfectants, and, above all things, to avoid any possibility of doing damage to the patients who employ them. Only this day I heard of a case which shows that a practitioner would do well to err on the side of overcaution. I was asking about a gentleman whom I knew some twenty years ago, who had settled down in practice in a large town. He was an able man, a gentleman in every sense of the word, of good character and great ability. I was told that he did not get on. I asked why, and it appeared that, in early life in that town, he was introduced to two families. He attended a lady in one of those families, who died of puerperal fever through no fault of his. He attended another lady shortly afterwards, and the people blamed him, and said that, having attended one woman with puerperal fever, he ought not to have attended a second, and he never got over it. I will not say that he deserved his fate; but certainly, if he had shown a little more caution, he would have proved the truth of the old maxim, that "honesty is the best policy." I believe the Fellows of the Society will do great good, not only to themselves, but to all whom they influence, if they inculcate, not carelessness, not protection of ignorant midwives, but instruction to those midwives that they must be over-cautious, and that, if they find themselves in danger of propagating or carrying about disease, they must err on the safe side, and say: "For a time I will give up practice; I will not run any risk of damaging the patient who has entrusted her life to my care."

OCTOBER 6TH, 1875.

WILLIAM S. PLAYFAIR, M.D., Vice-President, in the Chair.

Present—36 Fellows and 7 visitors.

Books were presented by Dr. Geo. Bland, Dr. H. F. Campbell, Mr. Benj. Clarke, Dr. A. Cordes, Dr. T. A. Emmet, Dr. F. Garrigou, Prof. O. Hasse, Dr. F. M. Madden, Prof. Schröder, Dr. S. S. Todd, and Dr. E. Van de Warker.

John Powdrell, M.R.C.S., and John Wallis Mason, M.R.C.S., were admitted Fellows of the Society.

Walter Kerr Waller, M.R.C.S. (Calcutta), was declared admitted.

Edward Chaffers, F.R.C.S. (Keighley), and Amos Rogers, L.R.C.P. Ed., were elected Fellows.

The following gentlemen were proposed for election: John Ford Anderson, M.D., R. S. Fancourt Barnes, M.B., John Branson, L.S.A. (Rotherham), and William Stewart, L.R.C.P. Ed. (Barnsley).

Before entering upon the ordinary business of the evening, Dr. Playfair alluded to the loss sustained by the Society, by the death of its Honorary President, the late Sir Charles Locock, who had rendered valuable aid to the Society, when it was first formed, by accepting the

Honorary Presidency, and by taking a special interest in furthering the aims of the Society. He announced that the unanimous feeling of the Council was in favour of asking Dr. Arthur Farre to accept the vacant post, which was received with acclamation by the Fellows present.

Dr. Gervis exhibited a pessary which had been retained many years.

F. W—, æt. 56, admitted July 6th. Has had five children, and youngest 17 years. Shortly after birth of last child, suffered from prolapsus, for which a pessary (gutta percha) was introduced, and allowed to remain fifteen months; and then exchanged for present one, which has not been removed from then till now, a period of over fifteen years. Four years since she began to feel a weakness in the lower part of her back; two years ago her legs became weak, and she became unable to walk without assistance; for six months she has been nearly bed-ridden. Her catamenia ceased ten years ago. A year and ten months ago she noticed a leucorrhœal discharge, and for now six months this has become considerable in quantity, occasionally tinged with blood, and highly offensive. She complains of no pain in micturition or the action of the bowels; her general health appears fair, and appetite good; but she can neither stand nor walk, and is able to move her legs, in bed with difficulty; three or four times a night awake by attacks of cramp in her legs. On examination per vaginam, *à lensei*, a large, round, metallic pessary was found imbedded in its walls, and considerable quantity of brownish purulent discharge of horribly offensive fœtor escaped. It was removed with the bone forceps, and the patient recovered perfectly.

Dr. Cleveland remarked that, apart from the interest attaching to the long impaction of a foreign body in the vagina, there was the notable fact that a quantity of highly offensive putrid matter must have been locked up for a considerable time, without the supervention of septicæmia. The case, it appeared to him, tended to show, in connection with the

recent debate before the Society, the necessity for the co-existence of some peculiar condition of the general system before absorption of morbid matter, followed by blood poisoning, could be effected.

Dr. Bantock related a case which came recently under his care at the Samaritan Hospital. The patient, æt. 70, came from the neighbourhood of Birmingham. She complained of pains in the hypogastrium, bearing down, &c., which were increased by exertion or the erect position, and a "bad discharge." There was a very free muco-purulent watery flux from the vagina, having a horribly offensive odour, and on examination he found a pessary of nearly the same size as the specimen shown, but oval in shape —composed of horsehair, covered by india rubber—which he had great difficulty in extracting. The vagina was studded with red points and was very tender. It was washed out with a weak solution of carbolic acid, and then mopped with the glycerine of tannin. A week after the vagina was whole, and the patient was dismissed with a Zwanke.

Dr. Rogers referred to a case he had before mentioned to the Society, of a boxwood pessary, which had been left in eighteen years, and had been forgotten by the patient. It was broken up with bone pliers and removed.

Dr. Edis also alluded to a similar case he had brought before the Society, where a large oval pessary, of the size of a goose's egg, had been left in the vagina for eleven years. The case had been supposed to be cancer. It was removed with some difficulty by forceps.

Dr. Playfair thought these archaic instruments would soon be out of use. He wondered why the ulceration had not extended into the bladder and rectum. As regarded Dr. Cleveland's remark, he had noticed that fœtid matter of intense virulence might be present, provided there was an absence of an absorptive surface of a freshly formed wound.

Dr. Gervis, in reply, stated that in removing the pessary a slight laceration took place. The fœtid matter passed over it, and yet there was no septicæmia.

Dr. Hayes exhibited a fatty placenta and a stillborn fœtus which had been expelled, a few hours before the meeting, from a syphilitic patient. The woman, æt. 22, was in the eighth month of her second pregnancy. Her first child was born alive, and quite healthy, but at the end of three weeks a rash appeared round arms and on head. A fortnight ago the fœtal heart was audible, but

the fœtal movements had not been felt during the past eleven days. The patient had on scalp and arms a few scattered spots of syphilitic psoriasis. There was a clear history of syphilis in her husband. Labour had occurred this forenoon (Oct. 6th). The placenta was of ordinary size and thickness. Its colour was generally pale and yellowish, in some parts worse than in others; its substance very soft and lacerable. Under the microscope the villi and vessels were found to be filled with fat globules. Fœtus corresponded to supposed time of gestation, and had probably been dead ten or eleven days. It exhibited externally no traces of syphilis.

Dr. Hayes remarked that this case gave rise to an important question; whether the disease of the placenta, and the death of the fœtus, were directly due to the syphilitic poison.

Report on Specimen exhibited by Mr. F. Wallace.—The specimen is found to consist of the uterus, left ovary, upper third of vagina, and a tumour. The uterus is enlarged to more than double its natural size. Its fundus and body are smooth and somewhat flattened, and are attached by their posterior surface and borders to the lower part of the tumour, which is here deeply hollowed out, as it were, to receive them. The connections of the tumour with the uterus are chiefly fibrous, and are easily broken through, except at one point near the internal ὸs uteri. The cavity of the uterus is proportionate to its size, and is no where narrowed. The uterine walls are thick, but appear quite healthy. At the lower part of the body on the right side, seemingly near the internal os uteri, the muscular tissue is seen gradually to lose its ordinary colour and become continuous with the tissue of the tumour. There is then formed a short narrow pedicle, which is surrounded by the capsule of the tumour. The

left ovary equals in size a large walnut, is cystic, and is firmly attached to the posterior border of the uterus. There is no trace of the right ovary or its ligaments. The tumour is round and fairly smooth on its surface. It is as large as an adult head. Its consistence in parts is firm and hard, in others soft; in some places there is almost the feeling of fluctuation. A very thick fibrous capsule surrounds the tumour, and is reflected along the pedicle on to the uterus. The dense and firm parts of the tumour creak under the knife, and consist of intersecting bands and laminæ of fibrous tissue. The softened and fluid portions of the tumour are chiefly near the surface, and are obviously the result of caseous and mucoid degeneration. On the whole, the softened tissue preponderates over the firm. The tumour, in our opinion, is a subperitoneal pedeculated fibroid undergoing caseous and mucoid degeneration.

THOS. C. HAYES, M.D.
J. H. AVELING, M.D.

Report on Mr. F. Wallace's Case of Monstrosity.—The fœtus was fully grown; the feet each possessed six normal toes, and each hand six digits, the supernumerary ones having no bone, and the one on the left hand being attached by a thin pedicle. The penis and tongue were found slightly developed, the latter being bifid. One eye was wanting, and the other in a very rudimentary condition. The scalp over the site of the posterior fontanel was distended to the size of a large orange, and was found to be filled with a firm fibrinous clot. The membranes were protruding through the fontanel into the sac, and had an opening at their extremity caused by rupture.

J. H. AVELING, M.D.
THOS. C. HAYES, M.D.

REPORT OF A CASE OF COMPLETE INVERSION OF THE UTERUS OCCURRING IMMEDIATELY AFTER LABOUR.

By Frederick H. Gervis, M.R.C.S.,
MEDICAL OFFICER TO THE HAVERSTOCK HILL AND MALDON ROAD PROVIDENT DISPENSARY.

Mrs. T—, æt. 41, was taken in labour of her sixth child on March 14th, about 11 a.m., and sent for me at 3 p.m.; the labour was natural but slow and tedious, owing probably to the large size of the child. When the head was well down on the perinæum, I gave her 3j Liq. Ergotæ, as the pains were flagging, and the child was born at 5 p.m. The uterus contracted firmly on the placenta, and there was very slight hæmorrhage, so I waited for fifteen to twenty minutes, and then a careful examination made it apparent that the placenta was attached to the uterine wall. I was in the act of baring my arm to pass it into the uterus, when my patient earnestly begged me not to do so, as she had suffered so much pain in a previous confinement from the same operation; and as she assured me that a strong pain was coming on, I passed my hand only into the vagina, which was very large and flaccid, and feeling the placenta projecting through the os, I caught it and drew it gently down, congratulating my patient on the ease with which it was coming away. It seemed to me to be unusually large and hard as it followed my hand, and it came so easily and gradually, that until it had passed outside the vulva, I did not at all realise that it was the uterus inverted and accompanying the placenta. It formed a hard, solid feeling tumour, about seven to eight inches long, and five to six broad, with the placenta adherent on the left side; this I immediately peeled off and removed entirely; there was not much general hæmorrhage, but one small artery spirted out per saltem, and was soon

stopped on pressure from my finger. As soon as I had removed the placenta I looked up at my patient and found her syncopic and apparently dying, cold, pulseless, and quite insensible. I at once gave a large dose of brandy, and repeated it as often as she could swallow it, and as soon as I could get it, five minims of Liq. Ammoniæ Fort. in mucilage frequently; in about ten minutes she rallied slightly, and I made the nurse continue giving the brandy and ammonia, whilst I tried to reduce the inverted uterus. My first attempt was apparently successful, for the fundus slipped back and the whole tumour disappeared into the vagina, but a careful examination showed that the re-inversion was only partial and incomplete. I continued my manipulations inside the vagina, but finding it impossible to complete the operation, I again drew it outside the vulva. I then steadied the os and cervical ring with my left hand through the abdominal wall, and kept steady pressure on the fundus with the tops of the fingers of my right hand close together, and after about ten minutes steady pressure, the fundus gradually retreated, and I could feel through the abdominal wall that the tumour was reinverted, and in its normal position. There was no special jerk or muscular movement at the moment of its return, but it went back as slowly and steadily as it came down, and the patient was fully sensible of its replacement. My friend Dr. Heath Strange, who kindly came to my assistance, applied one pole of a magneto-electric machine over the uterus, the other pole being at the back, and the uterus contracted firmly in its proper position. There was no hæmorrhage of any consequence during the operation, nor afterwards, but throughout the patient had repeated attacks of syncope, and remained very collapsed for the next five or six hours, so that I continued to give her five minims of Liq. Ammoniæ Fort. with ten minims of Liq. Secalis every half hour, besides beef tea and brandy. She passed a quiet night, and the next morning, about sixteen hours after the accident, I found her fairly recovered from the shock, free from pain

and in good spirits. I changed the ammonia draught, for one containing five minims Sp. Terebinth. with fifteen minims Liq. Opii Sed. in mucilage every four hours. I also washed out the vagina with a weak solution of Liq. Pot. Permanganatis. There was no abdominal tenderness or pain, no heat of skin nor rigors; urine had been passed freely, and the lochial discharge was perfectly natural; the pulse was high, 120 per minute; but she is an excessively nervous woman and generally has a rapid pulse. I continued to give her the turpentine and opium for a week, and also washed out the vagina twice daily; she progressed most favorably, without any bad symptoms whatever; the milk came in the breasts on the third day, the lochia passed through the usual changes, and she has made a very good recovery.

Remarks.—The rarity of this accident has induced me to place the above case on record. It is the first time I ever saw an inverted uterus, after fifteen years' large midwifery practice, and I sincerely hope it will be the last. The inversion appeared to me to be produced by a violent muscular spasm of the body of the uterus, assisted by the slight traction I had commenced to make, which I can conscientiously say was very slight and no more than might fairly be made. I can fully corroborate the remarks of writers on this subject as to the advantage, indeed I may say the *necessity*, of steadying the neck of the uterus through the abdominal walls during reduction, until I did this, the uterus was constantly slipping back *en masse* into the vagina; but when I once managed to get a firm hold with my left hand on the neck, I soon reduced the tumour by gently and firmly pressing on the base with the tips of the fingers of my right hand. The syncope was very persistent and alarming, and was evidently from shock to the nervous system rather than from hæmorrhage, which was certainly not excessive. I found five minims of Liquor Ammoniæ Fort. frequently administered, answered better than brandy as a stimulant, and being well sheathed with mucilage was borne well. In

the after treatment, I continued washing out the vagina with a diluted solution of permanganate of potash, and gave turpentine and opium rather as a preventive than a curative, for she really had no bad symptoms whatever; but I have so often found puerperal febricula, or slight peritonitis, or a too high temperature checked speedily by turpentine and opium, that I gave it in this case, fearing that there must be some inflammation of the uterus or its appendages set up after such severe handling as it had received. It was then, and is now, a matter of wonder to me, that so delicate an organ as the uterus could be turned inside out, and bear such necessarily rough manipulation in its reduction, and yet apparently be no worse for it. Seven months have elapsed since the accident, and my patient is in good health, with the exception of slight sciatica in the left hip.

Dr. Galton remarked that the mode of reduction mentioned by the author, although successful in this case, was one hardly to be recommended in general. The author describes the reduction as effected by pressing two or three fingers up into the fundus of the uterus. This plan would necessitate the passage of four folds of uterine tissue through the os, instead of two folds, which would alone require to be passed through, were the reduction accomplished by the more mechanically correct and usually recommended method of grasping the whole body and returning first the part latest in coming down.

Dr. Grigg inquired whether there had been any severe cramps and pains in the thighs as well as shocks. He remembered a case in his own practice where the uterus came down with the greatest ease, without any traction having been exerted.

Dr. Bantock thought the treatment was confirmed by the result. He did not agree with the preceding speaker in thinking that the uterus was doubled up by pressing on the fundus, for we were told that the body of the uterus felt hard. It appeared to him that the reduction began at the cervix.

Mr. F. Gervis explained that the uterus was exceedingly rigid and hard, and it was difficult to get a fold of the uterus. Whether the fundus or the cervix went up first he did not know. In a previous confinement, she had had severe cramps.

Dr. Cleveland thought the whole question of inversion of the uterus of such an important character that he hoped he might be

excused for inquiring more minutely as to the amount of traction on the cord that had been used. He could conceive that, owing to the administration of ergot before the birth of the placenta, especially if it happened to be adherent, the uterus might take on that irregular and spasmodic action which, with only moderate traction on the cord, might eventuate in inversion. He did not, however, for a moment doubt but that Mr. Gervis would satisfactorily answer his questions.

Dr. H. Gervis remarked, with reference to Dr. Cleveland's suggestion as to the production of inversion, that so accurate an observer as Dr. Tyler Smith had in one instance seen the uterus invert itself, and it was now generally allowed that this accident could arise quite independently of traction. While on the subject of inversion, he would like to ask whether any Fellow present had ever seen the uterus after inversion reinvert itself. He had read recently an account of such a case, but imagined it must, at all events, be an occurrence of extreme rarity.

Dr. Hayes observed that when it was considered what difficulty sometimes occurred in removing the placenta, the cord giving way first, it was scarcely probable that Mr. Gervis could have exerted sufficient traction to produce the inversion.

Dr. Aveling remarked that inversion might be caused by *visatergo* and even *post mortem* from the development of gas in the abdomen.

Dr. Grigg mentioned a case occurring five days after labour, where no ergot had been given.

Dr. Rogers had seen only one case, which he replaced by means of an air pessary.

Dr. H. Gervis referred to a case reported in one of the early volumes of the 'Transactions,' where the uterus had reinverted itself.

Dr. Wiltshire admired the courage and candour of Mr. Gervis in bringing forward his case. After fifteen years' experience as an accoucheur Mr. Gervis would scarcely use immoderate traction; still the process of inversion having commenced, such traction as was employed might have contributed to the result. The placenta should be peeled off and not pulled off.

ON THE OCCURRENCE IN NORMAL LABOUR OF LATERAL OBLIQUITY OF THE FŒTAL HEAD.

By A. L. Galabin, M.A., M.D.

LATE FELLOW OF TRINITY COLLEGE, CAMBRIDGE; ASSISTANT OBSTETRIC PHYSICIAN TO GUY'S HOSPITAL.

That elementary branch of the science of obstetrics which consists in ascertaining the mode in which the fœtus traverses the maternal passages may be regarded as now almost complete. The subject of the present paper is one of the very few points connected with it as to which doubt or difference of opinion still remains. The present aspect of the question is somewhat singular, for while the view first introduced by Naegele, that at the commencement of labour there is a lateral obliquity of the fœtal head in reference to the pelvic brim, has been generally accepted, and is to be found in most of our text-books; yet the opposite opinion is maintained by several of the most distinguished recent authorities on the mechanism of parturition, both in Britain and in other countries. Amongst these may be especially mentioned Dr. Matthews Duncan and Dr. Leishman. The same view is supported by Velpeau, Cazeaux, and Küneke, and in the recent systematic treatise of Schrœder the lateral obliquity is no longer included amongst the relations or movements of the fœtal head.

In considering this point it will be assumed that the fœtal head is in the first position, and that both the axis of the uterus and the resultant of the expulsive forces have that direction which is generally considered to be the normal one, namely, that both are perpendicular to the pelvic brim, and therefore inclined at an angle of about thirty degrees to the horizon. The question will remain for after examination, what modifications are introduced by any obliquity of the uterus or of the resultant force in reference to the brim.

To make clear the precise meaning which has been attached to the term obliquity, or lateral obliquity, I shall quote the words of Naegele, as rendered in the translation by Dr. Rigby of his original paper, "Ueber den Mechanismus der Geburt."

"At the entrance of the pelvis the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. The sagittal suture is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments."

Although it is not so expressly stated, it seems to be clear from the succeeding sentence that in this passage Naegele means to say that the right parietal bone is lowest or deepest, not merely in reference to the horizon, but in refereuce to the plane of the pelvic brim.

The view of Naegele is further explained in the following passages:

"The higher the head is, the more oblique is its direction; from which reason the right ear can generally be felt behind the pubes without difficulty, which would not be the case if the head had a perpendicular direction."

"On account of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never during its passage coincide with the diameters of the pelvic entrance."

It is easy to see that this lateral obliquity, as understood by Naegele, implies a left lateral flexion of the head of the fœtus upon its trunk at the commencement of labour. This, at a later stage, would be replaced by that right lateral flexion which must accompany the extension of the head as it approaches the outlet, unless it has been so completely rotated that its long diameter has taken an exactly antero-posterior direction.

Amongst the authors who describe the occurrence of lateral obliquity may be mentioned Dr. Tyler Smith, Dr. Meadows, Dr. Milne, Dr. Churchill, and Dr. Murphy. Of these, Dr. Tyler Smith and Dr. Murphy regard it not, as Naegele describes it, as a condition, the degree of which is greater the higher is the position of the head, but as a movement of rotation about the occipito-frontal axis, by which, during the earlier part of labour, the right parietal bone becomes progressively deeper in reference to the plane of the brim.

The alternative view, as maintained by Dr. Matthews Duncan and others, is that the head enters the brim directly, with its vertical diameter at right angles to the plane of the brim, and maintains this position in passing through the first half of the pelvic canal, whose shape is almost cylindrical. It will follow from this that the axis of the brim will pass through the sagittal suture, and that the same suture will always be equidistant from the upper margin of the os pubis and the promontory of the sacrum, until the head has decended so low that the resistance of the anterior curvature of the sacrum begins to effect a combined movement of extension and right lateral flexion. Not only, however, will the lowest point, in reference to the horizon, be from the first the right parietal bone, and not a point on the sagittal suture, but the examining finger will always first meet the right parietal bone; and thus the statement of Naegele, that the finger, introduced in the direction of the central line of the pelvic cavity, touches the right parietal bone in the vicinity of the tuber, will be true even though there be no lateral obliquity. Moreover, it is to be remembered that even in multiparæ, and still more in primiparæ, the presenting point of the vertex lies below the plane of the brim before the commencement of labour; and therefore from the first the sagittal suture will be nearer to the sacrum, although not to the promontory of the sacrum, than it is to the pubes. Furthermore, if, with Dr. Matthews Duncan, we define the presenting point as that point on the sur-

face of the child's head through which the curved axis of the pelvis passes, then the presenting point will never lie on the sagittal suture. If, however, with Dr. Tyler Smith, we define the presenting point as that portion of the head felt most prominently within the circle of the os uteri or vagina in the successive stages of labour, then if there be no lateral obliquity, and the os uteri be symmetrically situated about the axis of the uterus, the presenting point will be a point on the sagittal suture so long as the fœtal head remains within the os.

The settlement of the question by direct observation presents great difficulties, for it may be regarded as impossible to measure accurately the distance of the sagittal suture from the promontory of the sacrum and from the upper margin of the pubes, and it is scarcely easier to introduce the finger actually in the axis of the brim, and not merely in the direction of that axis. Dr. Leishman has endeavoured to substitute for the finger Professor Buchanan's rectangular lithotomy staff, but the difficulty must be great in securing that the arm of the instrument shall coincide precisely with an imaginary line. Most observers appear to agree with the statement of Naegele, that the sagittal suture divides the os uteri into two unequal segments, the anterior being the larger; but this might be explained on the supposition that the head enters the brim directly, but that the os uteri is displaced forwards. Perhaps the best mode of observation is first to form an estimate whether the os uteri is centrally situated with regard to the axis of the brim, and if it be so, to ascertain if it is divided into two unequal segments. From examinations made in this way I have been led to believe that not unfrequently a lateral obliquity of the fœtal head does exist. Since, however, such eminent authorities have arrived at such discrepant opinions, it may be concluded that this direct evidence may be satisfactory to the observer himself, but can scarcely be relied upon to convince others.

Nor is a much more conclusive testimony to be

obtained from a consideration of the caput succedaneum. The statement of Naegele on this point is as follows :

"Under certain circumstances, a swelling of the integuments of the head frequently forms soon after the os uteri has begun to dilate. This swelling is situated upon the right parietal bone, close to its upper edge, and equally distant from both angles : a small piece sometimes extends over the sagittal suture unto the other parietal bone ; its circumference depends upon the degree of dilatation which the os uteri had attained."

This description, with some slight modification, is accepted even by those who deny the occurrence of lateral obliquity, and it appears at first sight to be a strong confirmation of the view of Naegele. It is urged, however, by Dr. Matthews Duncan that it is a mistake to suppose that the thickest part of the swelling corresponds to the centre of the area upon which it is formed. He considers that the direction of the caput succedaneum of the first stage will be that of least resistance ; that is, the direction of the undilated vagina ; and that the support given by the posterior vaginal wall to the posterior half of the space enclosed in the circle of the os uteri, will cause comparative thinness of the swelling over the left parietal bone. This explanation is undoubtedly true as regards the caput succedaneum formed at a time when the resistance of the vaginal walls plays any part in the retardation of the head. Yet it is to be remembered that the swelling at one part is solely due to the pressure at other parts. If then, in the case of a multipara, the head is exposed at an early period of labour to the pressure of a rigid and undilatable os, and a caput succedaneum formed in consequence, it seems unlikely that, while the retro-cervical pouch of the vagina is still unobliterated, the pressure of the posterior vaginal wall can be sufficient to contribute to the formation of a caput succedaneum. I have thought that the situation of the swelling in some cases of this kind in which no displacement of the os uteri forwards could be detected, was in favour of the existence of lateral obliquity ; but it is so

difficult to exclude the effect of pressure exerted on the head at a somewhat later stage, that the evidence is not fully satisfactory.

Since, then, there is so much difficulty in settling the matter of fact, whether lateral obliquity occurs, it may throw light upon the question to examine whether there are any menchanical causes at work which would tend to produce such a result. This is the more desirable since it has been urged, as a very cogent argument against the existence of obliquity, that it is impossible to find a mechanism to account for it, and it has been said that the only conceivable cause for it would be a spontaneous movement of the child.

On the supposition that the head enters the brim obliquely, Naegele claims for this condition a mechanical advantage over the direct entrance. "On account," he says, "of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never during its passage coincide with the diameters of the pelvic entrance." Upon this Dr. Matthews Duncan remarks that it is useless to consider the advantages presented by an oblique or direct entrance of the head until the previous question be settled, whether the entrance be direct or oblique. It appears to me, on the contrary, that herein lies the most essential part of the argument, as far as regards the mechanics of the question. For not only might it *à priori* be thought probable, that if it met with any considerable resistance, the head would tend to take that position in which it would pass most easily, but there is no insuperable difficulty in understanding by what mechanical forces this result would be brought about.

For let us suppose that an oblate spheroid is pushed through a somewhat elastic cylindrical passage by a force applied to its posterior pole, its short diameter being in the axis of the passage. If the direction of the pushing force pass precisely through the centre of gravity of the spheroid, it may retain this position; but if it deviate in

the slightest degree from this, a rotation on a transverse axis will be produced, and the resistances at the end of the displaced diameter will then tend to increase the displacement. Again, if the body be an irregular ovoid, whose general shape is that of a prolate rather than an oblate spheroid, but if there be any one meridional plane, a section in which, near the points of contact with the cylindrical passage, approximates in shape to that of an oblate spheroid, then there will be a tendency to a similar displacement in that particular plane, if not counteracted by the resistances in other planes. The general principle may therefore be stated thus, that *if an ovoid body be pushed by its posterior pole through a passage like the genital passage, which admits it with difficulty, and if it be so shaped that a slight rotation on any transverse axis would allow it to pass with greater ease, then it will be in a position of unstable equilibrium in regard to the pushing force until such rotation has been accomplished, and the slightest variation in the direction of the force will bring about the rotation.*

Moreover, since the tendency of the pushing force by itself will be to increase any slight rotation which may have occurred, we must go further than this, and say that *there will be a tendency to displacement by rotation about a transverse axis, unless the shape of the ovoid be such, that the changed resistances called into play by any slight displacement have the effect of counteracting that displacement.* These principles may be applied to the fœtal head, since the unsymmetrical position of the condyles with regard to an antero-posterior plane will not affect the movement of lateral flexion which is now under consideration.

The next point is to ascertain whether a slight lateral flexion of the fœtal head will increase or diminish the diameter of the head presented to each diameter of the brim, for upon this, according to the principles just stated, will depend the question, whether the effect of the resistances will be to counteract or to promote any displacement in the direction of lateral obliquity. For this purpose I have taken measurements in various cases, both of

dried skulls and of fresh fœtal heads, some of the results of which are contained in a table appended. Considering in the first place the direct lateral section through the parietal tubera, it is chiefly of importance to compare with the biparietal the subparieto-superparietal diameter, for by a slight lateral flexion the latter would be substituted for the former. This was measured at an angle of about 20° with the biparietal diameter, between two points at equal distances of about ·6 inch above the left and below the right parietal tuber. I found it to be in all cases less than the biparietal, but the difference varied according to the amount of prominence of the parietal tubera in different heads, and was much greater in the case of a fœtus whose presentation had been pelvic, and whose head had suffered no distortion. The difference in this case was the greatest of any observed, and amounted to ·35 inch. Thus, although there is no section of the head, which, as a whole, resembles an ellipse having its short diameter at right angles to the brim; yet, in this direct lateral section, on account of the prominence of the parietal tubera, the curve near the points where it comes in contact with the brim approximates to this form. To use accurate language, the essential point is that the radius of curvature of the section has a minimum value at the extremities of its greatest horizontal cord, namely, the biparietal diameter.

I also measured the greatest of all cords drawn in the same direct lateral section in a direction parallel to the subparieto-superparietal diameter. This I found to be generally less than, in some cases equal to, and in only one case, in which the head was greatly flattened, greater than the biparietal diameter. Hence, considering this direct lateral section only, the effect of the resistances will be to promote a slight lateral flexion of the head, and that, in most cases, throughout the whole of its passage through the brim.

Since, however, the left parietal tuber is directed towards the sacro-iliac synchondrosis, and hence is subjected to comparatively little pressure, it is of less conse-

quence to consider the resistances at the ends of the biparietal diameter, than those at the ends of that oblique diameter of the head which coincides with the conjugate or shortest diameter of the brim, and is therefore subjected to the greatest pressure. This, which may be called the oblique biparietal diameter, passes nearly through the parietal tuber on the right side, and through a point just behind the coronal suture on the left. With this must be compared that diameter which would be substituted for it by a slight rotation of the head, namely, a line joining two points at equal distances above and below the two extremities of the oblique biparietal diameter. As there is in this case no bilateral symmetry, the length of the line, which may be called the oblique subparieto-superparietal diameter, varies according to the direction in which the rotation takes place. If it be such as to produce a left lateral flexion of the fœtus, then the length of the line brought into coincidence with the conjugate diameter of the pelvis diminishes as the angle of flexion increases up to nearly 30°; but the length of the greatest parallel cord diminishes only up to an angle of from 15° to 20°, and then again increases. Hence, there will be an immediate advantage in a flexion of nearly 30°, but the ultimate advantage will be greatest in a flexion of from 15° to 20°. It may be readily tested that, if the points of the callipers be fixed so as just to pass over the head in the oblique plane at this inclination, they will not pass over it at all in a perpendicular direction.

On the other hand, if the flexion be towards the right, the length of the oblique subparieto-subparietal diameter diminishes only up to an angle of from 10° to 15°, and then again increases; while the length of the greatest parallel cord, namely, that through the right parietal tuber, increases progressively from the first. Hence considering the effect of the resistances in this, the most important plane, the conclusion is that if any left lateral flexion be once commenced, it will tend to increase up to an angle of 20° or even more, and

continue, in some measure at least, throughout the whole of the passage of the head through the brim. On the other hand, any right lateral flexion will be promoted by the resistances only up to a very slight degree, and will be counteracted before the greatest diameter of the head can enter the brim. The effect of the resistances in other planes is less important, and will be found to coincide in its general effects.

It remains to be considered whether there is any reason why flexion should occur initially towards the left side, rather than towards the right. This would be the case, if there were any excess of resistance at the posterior part of the pelvis. The promontory of the sacrum ought not indeed to form any actual projection into the brim, but even the well-formed pelvis is somewhat heart shaped, and the sacrum lies within the circumference of a perfect circle equal in area to the pelvic brim, while the symphysis pubis lies outside it. Pressure is therefore greater at the posterior than at the anterior extremity of the conjugate diameter. It is true that this is compensated for by the excess of pressure at the anterior extremities of oblique diameters, so that there is no excess of frictional resistance at the posterior part in the plane of the brim. But either the sacrum itself, the last lumbar vertebra, or the intervertebral substance, often forms a prominence somewhat above the plane of the brim, against which the head might for a moment press and be retarded before it comes fully in contact with the whole circumference of the brim. The smallest effect of this kind would be sufficient to determine the direction of the obliquity.

We have next to consider the effect of any obliquity of the uterus, giving rise to a corresponding obliquity of the expulsive force. The vertical axis of the fœtal head being initially parallel to the axis of the uterus, it will follow that, before any resistances come into play, the parietal bone on the same side as the uterine obliquity will be lowest in reference to the plane of the brim. But the effect of the resistances, so far as they become

modified by the inclination of the expulsive force, will tend to do away with this obliquity of the head in regard to the brim, and to produce exactly the opposite. For although the pressure, and therefore the friction, is greater on that side of the passage towards which the expulsive force is inclined, yet, owing to the elongated shape of the head as a whole, the force due to the excess of perpendicular pressure on that side will meet the axis of the head at a point in advance of its centre of gravity, and so tend to produce a rotation on a transverse axis. It is thus that the obliquity of the latter half of the genital passage, in relation to the direction of the expulsive force, produces the extension of the head in the later stage of labour, notwithstanding that the friction is greater at the posterior part of the passage. Moreover, as soon as any lateral flexion has commenced, the pushing force applied to the posterior pole of the ovoid will tend to increase it.

At a time then when resistances have come into play, a right or anterior obliquity of the uterus will counteract, while a left or posterior will promote such a lateral obliquity of the head as Naegele describes. We may here derive one practical conclusion from a somewhat dry and unpractical subject. For the measurements of the fœtal head show that if the obliquity exceed a certain degree, no mechanical advantage, but, on the contrary, disadvantage is the result. If, therefore, in any particular case the obliquity be found to be excessive, producing an undue displacement of the sagittal suture backwards, and of the right ear downwards, it will be of advantage to place the woman on her right side, instead of on the left side or on the back. An obliquity of from 15° to 20°, which in most cases appears to afford the greatest mechanical advantage, corresponds to a displacement of the sagittal suture a distance of from ·4 to ·6 inch towards the left sacro-iliac sychrondosis.

The modification in the effect of the resistances by the

inclination of the expulsive force may be too slight to reverse the tendency which they would otherwise have to promote any slight obliquity in whatever direction it already exists, or has been produced. Hence, it is possible that the common right obliquity of the uterus may have a more important influence than the resistance of the sacrum in determining such a lateral obliquity of the head that its right side is deepest in the brim. For initially it will cause such an obliquity, and it has been shown that, in general, the tendency of resistances is, up to a certain point, to promote and increase any obliquity of the head. The inclination of the expulsive force will diminish this tendency in regard to the kind of obliquity at first existing, but, if slight, will probably not altogether cancel it, and in that case the obliquity of the head will increase or continue throughout the whole of its passage through the brim.

I have recently met with one case which appeared to me to afford positive demonstration, that, in that particular instance, lateral obliquity had existed. It was one in which the head was arrested, and the use of the forceps became necessary, from a cause which, I believe, is a very uncommon one, namely, from excessive flexion. The result of this was that the back of the neck could easily be felt behind the pubes, while a considerable portion of the frontal bone still remained above the brim. In consequence of this not only had a swelling arisen upon the scalp near its usual position, but a secondary caput succedaneum over the eyebrows had been formed above the level of the brim. This was bounded by a definite margin, indicating the line of contact with the brim, and this line, instead of being horizontal and parallel to the orbits, as it would have been if the head had been entering the brim directly, was inclined obliquely towards the right eyebrow.

I have also endeavoured to test the question experimentally, by the help of an apparatus, constructed by the direction of Dr. Braxton Hicks for obstetric demon-

strations at Guy's Hospital. This consists of a bony pelvis covered with india rubber, and fitted into a wooden model, which includes the spinal column, and the uterus, with one of its sides removed. In pushing fœtal heads of various sizes through this pelvis by a force applied to the body of the child, I found that, if they passed easily, no perceptible obliquity arose, but that, if there were great resistance, the right parietal tuber entered the brim somewhat before the left. The direction of obliquity in this case could only be due to a slight hitching of the head against the sacrum. I found also a confirmation of the effects already attributed to a lateral inclination of the pushing force, when combined with the action of the resistances, namely, that a force directed towards the left side of the pelvis caused the left side of the fœtal head to descend the most, and conversely.

The conclusions which I have endeavoured to establish in the present paper are the following:

1. That the question of the direct or oblique position of the head in regard to the brim may be reduced to a question of stable or unstable equillibrium, and that if any considerable resistance is met with just before the parietal tubera enter the brim, an exactly direct position is one of unstable equilibrium with regard to the pushing force.

2. That in consequence of the want of symmetry of that section of the head which coincides with the conjugate diameter of the pelvis, and is therefore exposed to the greatest pressure, the effect of the resistances will, in most cases, be to promote any left lateral flexion up to an angle of 15° or 20°, and throughout the whole of the passage of the head through the brim, but to encourage any right lateral flexion only to a very small extent, and for a very brief period.

3. That there are reasons for expecting that, in most cases, a lateral flexion will occur initially towards the left side, rather than towards the right.

I believe that observation confirms the conclusion derived

from a consideration of the mechanics of the case, and that while in easy labours, when the uterus is not displaced, no lateral obliquity of any importance occurs, and such little obliquity as there may be is variable in its direction; yet, if any considerable resistance is offered to the head just before it is fully engaged in the brim, a lateral obliquity arises in such a direction that the right parietal bone becomes deepest in reference to the plane of the brim.

Tabular Statement of Measurements of Fœtal Heads.

I. Fœtal skull in Guy's Hospital Museum. Left parietal bone much sheared backwards; sutures overlapping; parietal tubera somewhat prominent.

Biparietal diameter	3·60 inches.
Subparieto-superparietal diameter, inclined to biparietal at angle of 20°	3·35 „
Maximum parallel cord at same angle of 20°	3·55 „
Subparieto-superparietal diameter, inclined to biparietal at angle of 35°	3·25 „
Maximum parallel cord at same angle of 35°	3·65 „
Oblique biparietal diameter, coincident with conjugate diameter of pelvic brim	3·45 „
Oblique subparieto-superparietal diameter, inclined 15° to oblique biparietal, and brought into coincidence with conjugate diameter of brim by a *left* lateral flexion of fœtal head through 15°	3·35 „
Maximum parallel cord at same angle of 15°	3·35 „
Oblique subparieto-superparietal diameter, measured at an angle of 30° with oblique biparietal on same side	3·15 „
Maximum parallel cord at same angle of 30°	3·45 „
Oblique subparieto-superparietal diameter, inclined 15° to oblique biparietal on opposite side, and brought into coincidence with conjugate diameter of brim by a *right* lateral flexion of fœtal head through 15°	3·35 „
Maximum parallel cord at same angle of 15°	3·65 „
Oblique subparieto-superparietal diameter, measured at an angle of 30° on the same side as the last	3·45 „
Maximum parallel cord	4·20 „

II. Fœtal skull in Guy's Hospital Museum. Right parietal

bone slightly sheared backwards; sutures greatly overlapping, especially transversely.

Biparietal diameter	3·40 inches.
Subparieto-superparietal diameter, inclined 20° to biparietal	3·10 „
Maximum parallel cord at same angle of 20°	3·25 „
Oblique biparietal diameter, coincident with conjugate of brim	3·40 „
Oblique subparieto-superparietal diameter, brought into coincidence with conjugate of brim by left lateral flexion of the fœtal head through 20°	3·25 „
Maximum parallel cord at same angle of 20°	3·35 „

III. Fœtal skull in Guy's Hospital Museum. No shear of parietal bones.

Biparietal diameter	3.60 „
Subparieto-superparietal diameter, inclined 20° to biparietal	3·45 „
Maximum parallel cord at same angle of 20°	3·55 „
Oblique biparietal diameter, coincident with conjugate of brim	3·75 „
Oblique subparieto-superparietal diameter, brought into coincidence with conjugate of brim by left flexion of fœtal head through 20°	3·60 „
Maximum parallel cord at same angle of 20°	3·65 „

IV. Fresh fœtal head. Right parietal bone sheared backward. Head much elongated and flattened laterally.

Biparietal diameter	3·63 „
Subparieto-superparietal diameter, inclined 20° to biparietal	3·53 „
Maximum parallel cord at same angle of 20°	3·67 „
Oblique biparietal diameter	3·70 „
Oblique subparieto-superparietal diameter at inclination of 20°	3·57 „
Maximum parallel cord at same angle of 20°	3·72 „

V. Head of fresh fœtus, whose presentation had been pelvic.

Biparietal diameter	3·75 „
Subparieto-superparietal diameter, inclined 20° to biparietal	3·40 „
Maximum parallel cord at same angle of 20°	3·60 „
Oblique biparietal diameter	3·63 „
Oblique subparieto-superparietal diameter at inclination of 20°	3·50 „
Maximum parallel cord at same angle of 20°	3·54 „

Dr. PLAYFAIR thought the Society was very much indebted to Dr. Galabin for his erudite paper. The point at issue was one of

great interest: he had failed to satisfy himself whether obliquity really existed or not, or whether Dr. Galabin's suggestions were sufficient. Dr. Galabin had not mentioned a very elaborate treatise on the subject by Dr. Hodge.

Dr. Gervis quite concurred in all that Dr. Playfair had said as to the value of Dr. Galabin's paper. It was, however, difficult to discuss such a paper without reading as well as listening to it. It would appear to favour the opinion that there was no necessary contradiction between the two opposite views; but that obliquity of the head depending upon some undue pelvic resistance was essentially a character of a labour in some degree abnormal; while in perfectly normal labour the equillibrium between the propulsive force and the resistance of the parturient passage being, as Dr. Galabin expresses it, stable, obliquity did not occur. If further research proved this view accurate, it would account for much of the present divergence of opinion.

Dr. Galabin thanked the Society for their patient attention. The subject would have proved more interesting, possibly, with a fœtal skull and a pelvis, than the account of the dry mechanical details had proved.

NOTE ON A DISSECTION OF A UTERUS PREGNANT ABOUT THREE AND A HALF MONTHS, THE PLACENTA BEING PRÆVIA, AND FIBROIDS EXTENSIVELY DEVELOPED IN THE WALLS OF THE UTERUS.

By J. Braxton Hicks, M.D., F.R.S., &c.

The uterus had been slit up on one side from the os to one third upwards. Unfortunately it had cut through the placenta, which overlapped the inner os by a little more than an inch, and thus the question as to the non-existence of blood among the villi, as I have already insisted on in this Society, was unable to be proved by the specimen. Neither could the exact condition of the portion of the placenta opposed to the inner os in placenta prævia be determined, a highly interesting point, as suggested by Dr. Matthews Duncan.

I directed my attention to three points :

1st. Whether there was a circular sinus at the margin of the placenta.

2nd. The distribution of the curling arteries in the decidua serotina.

3rd. Whether there was any change induced by pregnancy in the fibroma.

1st. *The existence of the circular sinus.*—Careful examination in the position pointed out by various authors did not reveal any channel; nor was there any part about the margin which gave any pre-indication of any such.

Although this paper is intended only to show the condition at the three and a half months, yet I may perhaps be permitted to remark that one can hardly look upon the so-called "circular sinus" as a necessary part of the circulation of the placenta; for as Dr. Matthews Duncan has pointed out it is not continuous, but septa exist in it, which prevent blood from running freely along it. Indeed, it is difficult to understand from what venous branches it could arise, and in this dissection there was no indication to assist one in determining. There are many spaces in connection with the decidua, such as the processes into which blood easily runs, which may be readily, and I believe are frequently, mistaken for sinuses. It is therefore, I think, a point yet to be carefully examined, whether the circular sinus is a true vascular structure, or an adventitious one; or an interspace formed by the general growth into which blood runs, upon the rupture of the numerous vessels consequent on the placental detachment.

2nd. *As to the distribution of the curling arteries in the decidua serotina of the placenta.*—I took portions of the decidua serotina, and removing carefully the adherent villi, placed it under the microscope. I found that there was a copious network of blood-vessels, injected well with blood, rendering them very easy of observation. There was no division of the serotina into lobules, nor any signs of the

decidual processes, such as are evident at a later period, and this is in accordance with what I have seen in all specimens before and up to about the period of pregnancy of this case.

Now, the vessels of the ramifications of the serotina occupied, of the total area about one fourth. The supplying arteries (the future curling arteries), taking other structures into comparison, were not inordinately large: I mean, I consider that each must have been wholly expended in the ramifications, and there was no sign of a large branch proceeding to another destination.

When the villi were detached from the decidua serotina, they floated out in water like sea weed, without the slightest sign of any attachment of one branch to another.

The total expenditure of the curling arteries in the substance of the decidua serotina, I have already pointed out in my former paper, is to be noticed at full term.

The points of interest confirmed by this examination are:—1. The decidual processes, consequently the separation into lobules, commences later than the three and a half months; and 2. There is no proof that any branch from the supplying vessels of the serotina pass into the intervillal space.

3rd. *The changes in the fibroma.*—On placing sections under the microscope I found the fibres about four times larger than in the ordinary fibroma, very much more distinct, and separable from one another; a few had attained the size of the colossal fibre of the ordinary pregnant uterus.

Dr. Edis observed that if the fibres in the fibroma increased during pregnancy, there was no reason why they should not equally be involved in the process of involution following parturition, the same as with the ordinary uterine fibres.

NOVEMBER 3RD, 1875.

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., President, in the Chair.

Present—48 Fellows and 6 visitors.

Books were presented by Dr. D. W. Hammond, Dr. Madge, Dr. W. L. Richardson, Dr. Tilt, Prof. Rizzoli, the Royal Medical and Chirurgical Society, the Clinical Society, and the Manchester Medical Society. A set of the 'Opera Omnia Physico-Medica' of Dr. Fred. Hoffmann, printed at Geneva in 1760, in six vols. folio, was presented by Mr. Tho. Taylor, of Birmingham.

Matthew Reid, L.R.C.P. Ed., was admitted; and Richard B. Cole, M.D. (San Franisco); Edward Chaffers, F.R.C.S. (Keighley); and Amos Rogers, M.D. (Ottawa), were declared admitted Fellows of the Society.

The following gentlemen were elected Fellows of the Society: John Ford Anderson, M.D.; R. S. F. Barnes, M.B.; John Branson, L.S.A. (Rotherham); and William Stewart, L.R.C.P. Ed. (Barnsley).

The following gentlemen were proposed for election: Geo. R. Cundell, M.R.C.S.; Jos. Fred. Eyeley (Dursley); J. Jackson Gawith, M.R.C.S.; Abijah J. Miles, M.D. (Cincinnati); Reginald Vernon Musgrave, M.R.C.S.; Arthur Herbert

Orpen, A.B., L.R.C.S. Ed. (Ross); Ernest Louis Tyler Smith, M.A., M.B. (Hounslow).

The PRESIDENT, before proceeding to the business of the evening, read a letter from Dr. Arthur Farre, accepting the office of Honorary President, which had been unanimously offered him by the Council.

Dr. EDIS exhibited a hydrocephalic fœtus which had presented by the breech, and had caused an impediment to delivery.

The mother, æt. 34, had had seven children. The last catamenia appeared the beginning of March 1875, and as she was delivered on the 28th of October, the interval was scarcely eight months. Labour set in before noon; the membranes ruptured about 1 p.m.

On arrival, Dr. Edis found the body of the child expelled all but the head. There was no pulsation in the cord, and life had been extinct for over an hour.

On examining the abdomen, the uterus was found to be firmly contracted, and of such a size as to preclude the supposition of a second child being in utero, so that the question of locked heads of twins was at once negatived.

The previous labours had been normal, and an examination per vaginum failed to detect any deformity of the pelvis. The child being dead, and the case having been recognised as one of hydrocephalus, the head was punctured behind the right ear: a sudden gush of clear fluid confirming the opinion that had been formed. Labour was then completed without further trouble, and the patient did well.

The circumference of the fœtal head was seventeen inches. A pint and a half of fluid could be injected before distending the head completely. Examination of the placenta failed to detect any morbid appearance calculated to throw light upon the nature of the case.

CASE OF ABORTION AT SIX AND A HALF MONTHS, WITH GENERAL DROPSY OF THE FŒTUS.

By Protheroe Smith, M.D.,
PHYSICIAN TO THE HOSPITAL FOR WOMEN.

Mrs. S. F—, æt. 35. Always delicate, though having no serious illness. Commenced to menstruate at the age of thirteen, and has now been married thirteen years.

Utero-Gestations and Labours.

I. 1863.—Son born at full period; healthy.
II. 1865, April.—Miscarried at third month.
III. 1865, December.—Miscarried at third month.
IV. 1867, June.—Female child born at full period; healthy.
V. 1868, August.—Abortion of six weeks.
VI. 1871, September.—Female child full period; born jaundiced, and died in three days.
VII. 1872, November.—Female child stillborn at the twenty-sixth week of utero-gestation.
VIII. 1874, August.—Son stillborn at the twenty-sixth week, having been dead several days.
IX. 1875, October.—Son stillborn at the twenty-sixth week, being generally œdematous.

In the last two instances albuminuria appeared in the few last weeks of pregnancy, with slight anasarca of extremities and face.

In that of 1872, the patient having been confined in the country, I failed to ascertain if the urine was albuminous, but it was assumed to be so, and the dark appearance, as from mixture with bile (according to an intelligent friend of the patient), was as in the two subsequent pregnancies. In

these three instances there was some œdema of the face and extremities, which as well as the albuminuria disappeared always in a week or ten days after labour.

The patient has been often the subject of disorder of the liver, and especially in utero-gestation, evincing itself by motions of a light and occasionally of almost a white colour; at the same time as pregnancy progressed the skin became more yellow, and in the last instance the jaundice existed for five or six days after delivery.

In the last pregnancy but one (1874), she almost entirely kept on the sofa, and grew very stout, especially about the neck; she was on full diet with wine.

In her last pregnancy (1875), she took gentle exercise out of doors, eat and drank considerably less, and was much thinner. About a week before her confinement she became suddenly (in one day) very large, and the face much flushed, which flushing continued till a week after her delivery. The pulse the last two months of her pregnancy was small, wiry, and incompressible, and her lips bright crimson. The symptoms which ushered in her labour were:

I. Bilious derangement.

II. Albuminuria about ten weeks before her confinement, yielding to small doses of Hydr. cum Cretâ, &c. At the same time this condition was accompanied by labour pains for which opium was prescribed. The albumen reappeared about five days before labour, and diminished with sudden increase of size, and on the appearance of some blood from the vagina three days subsequently, followed by watery discharge, which preceded labour pains twenty-four hours.

Considerable quantity of blood was lost before and after the expulsion of the placenta, owing to atony of the uterus, which required the incessant grasp of the hand for two hours and a half to prevent the frequent attempts of relaxation and hæmorrhage, notwithstanding the free use of injections of cold water into the vagina, and ice pessaries and ergot. This atonic condition of the uterus,

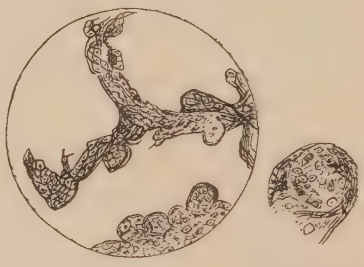
and disposition to post-partum hæmorrhage existed also in her previous labour, though to a less extent.

The incompressibility of pulse ceased after the birth. The flushing and œdema disappeared after the third day. The urine, which contained a large quantity of bile as well as about one fifth of albumen at the time of the labour, assumed its normal condition in about a week or eight days afterwards, and the patient is doing well, though for the first four days after her confinement she had pain in the left iliac region, and some tympanitic distension of the abdomen, but these symptoms soon yielded, and her present condition ten days after delivery is in every respect favorable.

When the child was born it appeared to be well developed for a six-and-a-half months' pregnancy. The head and face were of a dark purple colour, and together with the whole of the body were anasarcous to an extreme degree. This existed to such an extent that the presenting head was at first mistaken for the nates. On opening the abdomen the peritoneal cavity contained a large quantity of clear brownish-yellow fluid, with strings of lymph attached to the peritoneal surface of the liver, and floating in the fluid. The intestines were very small, and contained isolated portions of meconium. The peritoneum was apparently healthy. The kidneys were normal, and the epithelium in the uriniferous tubes was strongly marked, and had all the appearance of being healthy. The bladder was normal and empty. The lungs and heart were healthy, but each pleural cavity, as well as the pericardium, contained a considerable quantity of clear slightly coloured serum. The liver was about the usual size, perhaps a little smaller, of a natural brown colour, but soft and easily broken down with the finger; there was no appearance of congestion, and only a small quantity of blood escaped from the torn portions. Under the microscope no well-formed liver cells were perceived; the nuclei existed in abundance, but the cells themselves were very generally broken up,

as only the remains of them were found. The appearance they presented has been very correctly represented in the accompanying sketch by Dr. Heywood Smith. The

LIVER CELLS.



PLACENTAL TUFTS. One extremity more highly magnified.

placenta which came away was large, and consisted only of the fœtal portion. The placental tufts under the microscope were thin and badly developed, but regular, and the epithelium at the extremities ill defined and insufficiently developed. It is well to mention, in conclusion, that other members of this patient's family have suffered from considerable hepatic derangement at different times, which renders it not improbable, as this lady has also frequently suffered from similar hepatic derangements, that all these abnormal changes may have had their origin in some derangement of her hepatic system.

It is not improbable that the albuminuria and anasarca, the flushing of the face and crimson lips, the sudden increase of the liquor amnii, the escape of blood and serous discharge, and some slight confusion of ideas which existed just before the labour, indicated a condition of general hyperæmia, and may fairly be regarded as a natural effort to relieve the overcharged vascular system. May it not also be inferred that the diseased condition of the ovum may have resulted from this primary general derangement of the mother ?

A CASE OF GENERAL DROPSY IN A FŒTUS.

By Lawson Tait, F.R.C.S.

On June 17th, I received a fœtus from Mr. Hall-Wright, with the following history:

The mother was thirty-six years of age, and had borne six children previous to the one now in question. During the last months of this pregnancy she noticed that she was much larger than usual, and at the time of her confinement she reckoned that she was only seven months gone. For about twenty-four hours before the labour she passed large quantities of urine at frequent intervals. When called to the case, Mr. Hall-Wright found the os dilated widely, and the membranes protruding. Ballottement was very marked, and judging that he had to deal with an unusually large quantity of liquor amnii, Mr. Hall-Wright placed the patient in the erect position, with a vessel between the thighs, and ruptured the membranes. With the first gush the vessel was nearly filled, and a large quantity was lost. After the escape, some difficulty was experienced in determining the presentation, and it was supposed to be the breech. It turned out, however, to be the head, the mistake being due to peculiarities to be afterwards described. When the child was born it made several efforts to inspire, but did not seem to succeed; and though the heart was beating, all efforts to establish respiration were unsuccessful. There was no albumen in the urine of the mother.

I examined the fœtus and placenta within four hours after the birth, both being perfectly fresh. The fœtus presented a very singular appearance, its skin being so tense that it was not possible to bend the limbs without risk of bursting the skin. The fœtus seemed large enough, and heavy enough to be a child of the full time, but the condition of its development was such as made

it certain that the mother's reckoning was correct. The scalp was so distended that the bones could nowhere be felt. The eyelids, lips, and ears were so œdematous as scarcely to be recognisable.

When the skin was cut into, large quantities of serum oozed out, and an incision twenty-three millimètres deep had to be made through the skin of the forehead before the frontal bone could be exposed. The abdomen was enormously distended by highly albuminous fluid; indeed, so great was the amount of coagulable material present in all the dropsical fluids met with that when placed in a test tube, and the tube placed in boiling water, a solid cast was obtained. There was no appearance of peritonitis, nor of inflammation of any of the other serous cavities; yet both pleuræ and the pericardium were fully distended with fluid, and the scrotum was distended to hardness by a double hydrocele, quite separate, on both sides, from the peritoneal cavity. A small quantity of urine was found in the bladder, but it contained no trace of albumen.

The brain did not seem to be œdematous, but this is really not a matter on which I could hazard an opinion, as I can form no idea of what the appearances of dropsy of the brain substance in a seven months' fœtus would be. The ventricles did not contain any appreciable quantity of fluid. The muscular and subcutaneous tissues were uniformly very œdematous. The liver and kidneys, being the organs which at once suggested themselves as possibly the seat of some explanation of the singular phenomena, were examined by every method with which I am acquainted, but they did not yield any indication of pathological change. The heart and lungs I entrusted to my friend Dr. Sawyer, Physician and lately Pathologist to the Birmingham Hospital for Sick Children, knowing that he had paid a special attention to the conditions of the foramen ovale. He has favoured me with the following most interesting report:

"I have examined the heart and lungs you left with

me to-day. In my opinion the right lung has incompletely expanded, while the left lung has never expanded at all. There is no *direct* opening between the auricles through the foramen ovale. The valve of the foramen is so extremely developed that when viewed from the right auricle it forms a complete floor to the foramen ovale, giving it the appearanne of the fossa ovalis of the adult heart. But the occlusion is not perfect. Under the annulus ovalis, and completely under the cover of its upper and anterior edge, a small valvular opening leads into the left auricle, this opening being one twelfth of an inch in its largest measurement, and crescentic in shape with the convexity of the crescent directed backwards and towards the left. The septum ventriculorum is perfect, and the ductus arteriosus is fully as wide as the main trunk of the pulmonary artery.

"I have especially examined the condition of the foramen ovale in a large uumber of children's hearts, and I have very frequently found the closure of the opening no more complete than in this case, even in children who had completed their first dentition, and in whom no signs of admixture of the blood of the right and left sides of the heart could be discovered.—James Sawyer, M.D."

The placenta was large and very œdematous; but after being injected and carefully stained it displayed no further abnormality. The cause of the general dropsy seems therefore to exist in the premature closure of the channel between the auricles; a condition which would lead to a hindrance of the passage of blood from the right auricle and to a subsequent engorgement of all the vessels in front of the auricle throughout the double circulation. Thus the placenta was involved, and the hydramnios was induced, leading up to the premature birth of the child. From the passive obstruction in the placenta, it is probable that there was a general distension of the venous system resulting in the universal œdema. The duration of this disturbance of the circulation could not have been long, otherwise it is scarcely likely that the child should

have been born alive. The cause of the premature occlusion it is hopeless to seek for, and the case can only be referred to that rare class of malformation due to hypererchetism.

I have failed to find any record of similar appearances in a fœtus.

The discussion on the two foregoing papers being taken together,

Dr. Madge said he could hardly accept Dr. Protheroe Smith's explanation as to the cause of the death of the fœtus. He had given considerable attention to the subject, but he had never understood that a common disease or derangement of the liver in the mother could affect the child *in utero* in a somewhat similar way. It was well known that the mother may be morbidly affected in many ways, even with albuminuria resulting in convulsions, and still give birth to a perfectly healthy child. A syphilitic history—if such existed—might afford an explanation. The fœtal liver receiving the tainted blood direct from the mother became affected with syphilitic inflammation and deposit, leading to obstruction of the portal system and ascites.

Dr. Routh inquired whether any specific examination of the foramen ovale of the first child had been made.

Dr. Heywood Smith said he would be glad if any Fellow of the Society could throw any light upon the intermittence of the albuminuria; for in the attack the patient had two months before the labour set in, when there were symptoms of abortion associated with a sharp attack of albuminuria, the symptoms of abortion subsided under treatment, and the albuminuria decreased to a mere trace, although the uterus was increasing, and therefore the albuminuria could not have been in any wise due to increase of pressure.

Query as to heart.—Dr. Heywood Smith has since the meeting examined the heart and found it perfectly normal.

Dr. Snow Beck remarked that, through the kindness of Dr. Protheroe Smith, he had had the opportunity of examining each of the fœtuses he had referred to. He (Dr. Beck) had also had the opportunity of examining two other fœtuses which presented very similar appearances and had been expelled about the same period of pregnancy. The clinical history of each of these cases was very similar. At first two children were born at the full period, and grew up fine, healthy children. Then at the third, fourth, and fifth pregnancy, the fœtus appeared to die about the sixth month, and in a short time was expelled dead, as yet in no way decomposed, and presenting the evident signs of previous and serious disease. Great effusion of blood had taken place in the

subcutaneous cellular tissue over the whole of the body, a large amount of dark-coloured transparent serosity, with some strings of lymph floating in it, filled the peritoneum; whilst a small amount of slightly-coloured transparent serosity existed in each pleura and in the pericardium. This effusion of fluid was much too large to be the result of any change after the death of the fœtus, which, moreover, did not present the least evidence of any decomposition. Further than this condition of general dropsy, no change from the healthy state was detected in any of the organs except in the liver, which was somewhat paler than natural, soft, readily torn with the finger, with the liver-cells much broken down, leaving the large nuclei in many instances detached and floating in the field. It was a great puzzle to know how this state of disease could have arisen. The parents were singularly free from even the suspicion of a syphilitic taint, and the explanation which had been offered, that it depended upon some mechanical impediment or derangement of the circulation, could scarcely be accepted. The effused fluid was in too large an amount and too generally diffused to be caused by any mechanical disturbance of the circulation; nor did any of the organs present any evidence of congestion, either wholly or partially, which, he assumes, would have been the case had this condition depended only on mechanical disturbance of the circulation. The heart also was pale, with little or no blood in any of the cavities. The only probable explanation of this condition of the fœtus appeared to be that it arose from some deficient or altered state of the nutrition. But whether this arose from some want of formative power inherent in the fœtus which prevented its assimulating the nutriment furnished to its own tissues, or whether it depended on some condition of the mother's health which induced some defect or deficiency in the bood furnished to the placenta, and from which the fœtus derived a nourishment in some way unsuited to its healthy nourishment and growth, it appeared impossible to determine with the facts at present known.

The President said that in considering the pathology of these cases it was well to bear in mind that the subcutaneous cellular tissue in children after birth was prone to become œdematous from the infiltration of serum in certain depressed conditions of health. It was no uncommon occurrence in hospitals to find children, who had been ill-fed and exposed to adverse influences, the subjects of a general œdema which might proceed to an extreme degree, without such organic malformation or disease as to account for it. The villi of the placenta in Dr. P. Smith's case had the appearance of being atrophied, and he might suggest that possibly the œdema in the child was due to its imperfect nutrition *in utero*, and to the passive congestion thus induced.

Dr. Protheroe Smith, in reply, stated that there had been no unusual pressure on the funis; that the stillborn fœtus, though generally œdematous, appeared to have died but recently. The fœtal heart had been distinctly heard in the evening previous to the labour. It appeared to him that there was a great difference between his case and that of Mr. Lawson Tait, which, from the evidence adduced, was evidently a disease, primarily and altogether, of the ovum; whilst he believed in his (Dr. Protheroe Smith's) case the fœtal disease had originated from the disordered health of the mother. Having witnessed in a few cases, and especially in two instances, frequent consecutive abortions at about the fifth and sixth month of utero-gestation arrested by venesection, and followed, in succeeding pregnancies, by living children, he was desirous to inquire if, in the experience of any of the Fellows, similar results had been observed. He did not hesitate to say, after due consideration of the peculiarities of the case, that he believed the primordial affection was hyperæmia of the mother, who was predisposed to hepatic derangements. Hence the bilious disorders and jaundice, albuminuria, loss of blood, and serous discharge from the uterus, which had so repeatedly attended utero-gestation, though all this might be regarded as a natural but insufficient attempt at depletion; yet it ushered in a train of abnormal changes injurious to the healthy relationship between the fœtus and mother. The idea of hereditary taint was precluded by the fact of there being two living and healthy children, and by the history of the case. It may be well to mention the following curious impression of the mother, who was wholly ignorant of the state in which the fœtus had been born. She observed to her husband on the day following her confinement:—"I have been considering the circumstances of my sudden increase in size and the greater difficulty of this labour from the larger size of the child, and I think it must have been occasioned by the child becoming generally dropsical just before its birth."

ON A NEW FORM OF BLUNT HOOK AND SLING FOR ASSISTING DELIVERY IN CASES OF BREECH PRESENTATION.

By Joseph Griffiths Swayne, M.D.,
CONSULTING PHYSICIAN-ACCOUCHEUR TO THE BRISTOL GENERAL HOSPITAL.

Although labours in which the breech presents, as a general rule, require no interference until the lower half of the child's body is expelled, yet cases occasionally occur in which the breech is arrested for many hours in the upper part of the pelvic cavity, and an urgent necessity arises for some kind of assistance. The forceps, however, which is so serviceable under similar circumstances when the head presents, is, in these cases, worse than useless. It is sure to slip, for the body, unlike the head, is irregular in shape and deficient in that degree of resistance which is necessary to afford a firm grasp to the instrument. Even if a firm hold could be obtained, the use of the forceps would be attended with much danger to the child. There would be much risk of compressing the cord, and of squeezing either the thorax or abdomen between the ends of the instrument in such a manner as to injure important viscera, such, especially, as the heart or liver. As, for such reasons, the forceps is inadmissible in this form of difficult labour, Dr. Barnes recommends "breaking up or decomposing the obstructing wedge" by bringing down one foot or leg, and this undoubtedly is the best plan to adopt when all other expedients fail. But this operation is painful and difficult, especially if, as often happens, the feet of the child are high up and near the fundus uteri. It then, indeed, is more difficult than an ordinary case of turning would be, even long after the waters have escaped, for the hand has to be passed much farther to reach the feet. It is, therefore, better first to try some other means of delivery before having recourse to this expedient. In nearly every case traction would, doubtless, be sufficient if

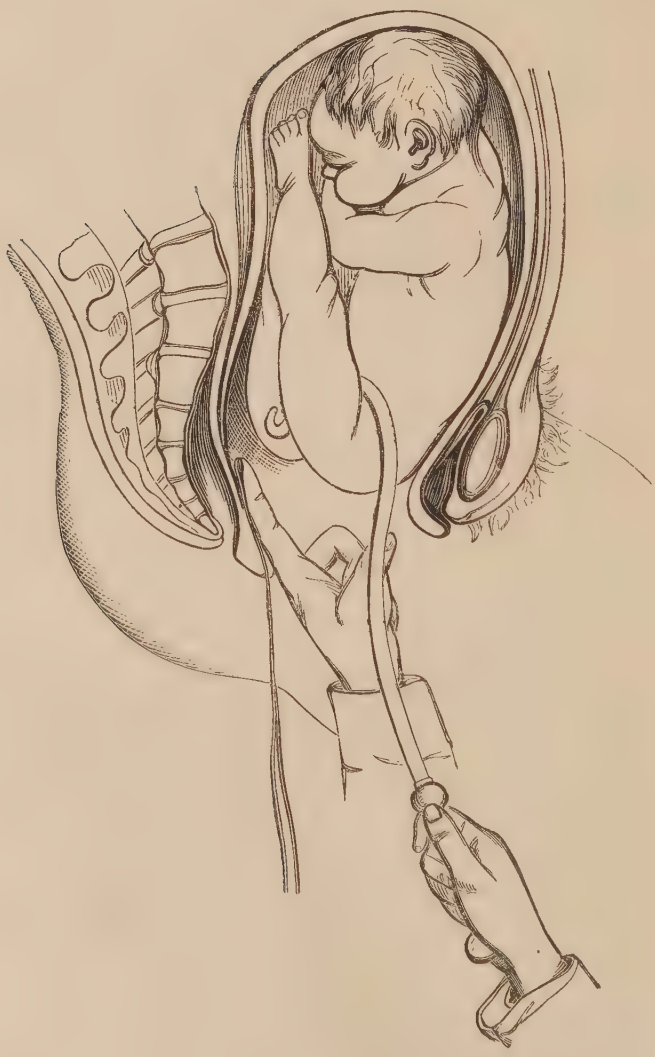
we could employ it as advantageously as we do when we get a firm hold of the head with the forceps. But this is not so easy to accomplish. A very simple mode of applying it is to pass the forefinger between the child's thigh and anterior superior spine of the ilium so as to hook it in the groin. In this way, if the pains are strong and the breech tolerably low in the pelvis and not very firmly wedged, a sufficient amount of assistance may be given to complete the labour; but if, on the contrary, the pains are weak, and the breech high and firmly fixed, this plan will generally fail, for the finger can exert very little power and soon becomes fatigued. In such cases recourse is often had to the blunt hook, but if much force be employed the instrument is hazardous to the child. It may bruise or tear the genitals and femoral vessels, or even fracture the shaft or detach the epiphysis of the femur. Assistance may be rendered quite as effectually, and much more safely, by some kind of sling; for instance, an ordinary piece of broad tape, ribbon, or bandage may be passed up by the finger round the upper part of the child's thigh: an expedient which I have myself adopted with complete success. It is, however, sometimes by no means easy to pass the loop round the thigh by the fingers alone. I have therefore, to meet this difficulty, proposed a modification of the ordinary blunt hook. This instrument is a blunt hook of the usual shape, except that the extremity is curved back upon itself, so that a loop may be hitched upon it. It may be used as a blunt hook, but it is chiefly intended for the introduction of a sling, which consists of a piece of strong silk cord with a loop at each end. This is padded in the centre and coated with india rubber. The handle of the hook is made to screw on and off, and also to fit a vectis, which may be carried in the same case. In order to apply the sling the extremity of the hook should be passed round one or, if possible, both thighs of the child, and whilst the handle is held steadily in the right hand one of the loops at either end of the sling is carried up on the forefinger of the left hand and hitched

upon the extremity of the hook. The hook is then withdrawn, but in doing so it should not be drawn directly downwards. If it has been introduced from the front, the handle should be carried backwards towards the sacrum of the woman; and if from behind, forwards towards the pubis. There will then be no danger of unhitching the loop until it is sufficiently low to be seized by the fingers.

Blunt Hook.

Sling.

Mode of using Sling when applied.



Introduction of Sling.

As soon as the sling is passed round the thigh of the child and both loops are drawn out, the handle of the blunt hook should be unscrewed and passed through them. We shall thus be enabled to get a good grasp and to make powerful extractive efforts in concert with each pain.

I should add that the instruments exhibited have been made by Messrs. Ferris, of Bristol, in accordance with my directions.

Dr. Aust Lawrence (of Clifton) showed the instrument for his colleague Dr. Swayne, and demonstrated its action.

Dr. Braxton Hicks thought that in the use of the blunt hook sufficient attention had not been given to the assistance afforded by external pressure applied to the fundus uteri in breech cases, and the delivery of the aftercoming head through the brim. He thought half the strain is taken off the thigh, and that without undue crushing of the uterine wall by the hand pressure. He thought that the text-books generally omitted to notice the great help given, and although he had no doubt some practitioners employed external pressure, yet very frequently it was never thought of, but all the force was put upon the hook.

Dr. Madge said he had had reason, like Dr. Swayne, to be dissatisfied with the use of the blunt hook for aiding delivery in breech-presentations; but he was afraid that in any case when much force was required some injury must be inflicted, whatever the plan adopted. He had suggested some time ago, with the view of protecting the soft parts, that the ordinary blunt hook should be covered with some soft material, such as india-rubber, compressed sponge, or wash-leather. If this could be done it would probably be a safer instrument than Dr. Swayne's sling, and would have the advantage of being much more quickly and easily applied.

Dr. Aveling observed that the life of india rubber was only about three years; it then became brittle, and if put aside it would be found useless.

Dr. Godson agreed with Dr. Aveling that whenever a foot could be brought down it was the plan of treatment which should be adopted, but there were occasionally cases in which it was impracticable. He had lately been called upon to deliver a patient in whom there was an impacted breech. Although able to seize a foot high up at the fundus, he found it impossible

to flex the knee, and he was obliged to have recourse to the blunt hook. He made use of that known as Lazarewitch's, and though every care was taken in the extraction, and the child was born alive, the skin in the inguinal region was cut through, and there was considerable bruising of the surrounding tissues, with much subsequent swelling of the thigh. He had since found by experiment that the bulb at the extremity of the hook was the point which receives the force applied, and pressed severely upon the tissues. He believed, therefore, the instrument to be a dangerous one, and warned the Fellows against employing it, as they might be led to do from the way in which it was recommended, being a component of one of the obstetric bags advertised.

Dr. Aust Lawrence had seen a case where the saphena vein was evidently lacerated by a blunt hook.

ON PROLAPSE OF THE FUNIS DURING LABOUR.

By George Roper, M.D., M.R.C.P.

PHYSICIAN TO THE EASTERN DIVISION OF THE ROYAL MATERNITY CHARITY.

Prolapse of the funis during labour is rarely primary: it is mostly the result of some other abnormality. These abnormalities may be divided into two kinds: the one comparatively unimportant; and the other of serious consequence.

The first class, induced by minor causes, includes the early rupture of the membranes with an immoderate quantity of liquor amnii, by which the cord becomes washed down. An unusually long funis or a funis given off from the lower edge of the placenta; a low position of the placenta itself in the uterus; an oblique position of the uterus; an oblique position of the child, not being a cross presentation so as to require turning; a wide and shallow pelvis, or a pelvis distorted at its brim, yet not so much so as to prevent the birth of a living child. All these abnormalities may cause prolapse of funis, but are

not so important as to demand any special treatment in reference to themselves, nor is their existence incompatible with the birth of a living child. Our attention, therefore, in these cases, may be directed to the management of the cord itself, and the conduct of the labour, with a view to prevent obstruction to the circulation of the cord during delivery. Whenever the cord is detected over the os uteri at an early period of the first stage of labour, before the membranes have ruptured, there cannot I think be a question about the propriety of version by the bipolar method; this will end the difficulty. Most frequently, however, we are not called to a case of this kind till the first stage of labour is advanced, and the liquor amnii discharged, the presenting part somewhat pressed down on the open os, and a coil of pulsating funis in the vagina. Our first object is to replace the funis within the uterus and retain it there; the case is then reduced to one of natural labour. The cord may be carried up into the uterus beyond the presenting part between the ends of two fingers, or by one of the many instruments used for this purpose. The postural method has also been advised. Whatever mode of replacing the cord we may adopt, it will be found there is no difficulty in this, it may easily be carried up above the presentation during the uterine pause; the real difficulty consists in keeping the cord within the uterus after it has been returned; the next systole is almost sure to force it down again. For the purpose of retaining it within the uterus, cotton wool or sponge has been used, but of this I have had no experience. After reduction, the best means of preventing its descent seem to be in securing the firm adaptation of the presenting part to the os uteri so as to leave no space between the edge of the os and the presentation through which it can pass. Pressure by the hands on the fundus uteri, exerted in a downward direction, will tend to secure this object. This method failing; when the os is sufficiently dilated, the forceps may be used with almost certainty of success. The funis being reduced, the presentation can be drawn

down on the open os so as to completely occlude it and prevent further descent. Where the liquor amnii has been discharged some time, and the bipolar method of version cannot be well practised, we have still a choice between the ordinary mode of turning by introducing the hand and the use of forceps. Of the two, I prefer the use of forceps, though in my own experience it is a clinical fact that version by introducing the hand has generally been found very easy. On passing up the cord between the two fingers, a knee or foot may frequently be felt and easily hooked down by the same fingers. I have frequently turned in this manner, when replacing a cord, without originally having any intention of doing so. Apart from the desirability of expediting delivery, for the safety of the child, these minor abnormalities, associated with prolapse of the funis, have a strong tendency to retard uterine action, and give rise to painful and lingering labour; by artificial aid the mother is thus spared protracted suffering. The treatment of prolapsed funis in this class of cases may be thus summarised:

1. When detected over the os uteri before discharge of liquor amnii—bipolar version.

2. When the labour is advanced and the liquor amnii some time discharged, after reposition of the cord within the uterus:

(*a*) Pressure on the fundus uteri in a downward direction.

(*b*) Use of forceps.

(*c*) Turning by introducing the hand.

In choosing between forceps and version I prefer the former; their use produces far less shock to the mother than turning, and in most respects turning may be considered to be liable to more risks than the use of forceps.

The other class of cases, in which there is prolapse of the cord, embraces those complications which are of far more serious importance than the prolapse of the cord itself, and our attention must be more particularly di-

rected to the treatment of these complications themselves. These serious causes of prolapse are malpositions of the child, which require turning or instrumental interference for delivery; deformity of the brim of the pelvis, or placenta prævia. In some of these cases, the treatment which overcomes the greater difficulty also saves the prolapsed cord from hurtful pressure, and a living child is born. Sometimes, however, these complications are not consistent with live birth, the necessity for operative procedure on the child, to effect delivery, being fatal. It is fortunate that the life of a child is but very rarely indeed sacrificed by any obstetric operation, because where there is a necessity for embryotomy, the combination of some difficult complication with prolapse of the funis will most likely have extinguished fœtal life.

In a case of contraction of the conjugate diameter of the brim, it not infrequently happens that a loop of funis drops down on either side of the projecting promontory of the sacrum. This, if pulsating, may be returned, and delivery attempted by forceps or by turning. The same may be said of prolapse of the funis associated with placenta prævia. Our treatment must be directed against the greater difficulty.

The following six cases, of which brief reports are given, occurred in ten consecutive months in the eastern division of the Royal Maternity Charity, in about two thousand deliveries. It has been thought not necessary to give reports of the first class of cases: they are self-evident.

Case 1, June 1st, 1874.—A multipara. Funis prolapsed with child's abdomen presenting; liquor amnii discharged twenty-three hours; funis pulsating; turned easily by introduction of hand; child very small and ill-nourished; born with heart beating, but natural respiration could not be established. Sylvester's method of respiration was tried for a long time without success; mother made a good recovery.

Case 2, November 5th, 1874.—A primipara, æt. 23, of

small stature, with diminished conjugate diameter of brim; had been in labour ten hours; liquor amnii discharged; the right hand and funis could be felt beside the presenting head; the funis was flaccid and pulseless. Long forceps were tried unsuccessfully. The head was then perforated. Head resisted delivery by craniotomy forceps. The cephalotribe reduced the base of the skull, and with this, as a tractor, delivery was easily effected. Dr. Bate, one of my colleagues in the charity, was present, and kindly gave chloroform. Mother made a good recovery.

Case 3.—A multipara, æt. 37; had been in labour ten hours; liquor amnii discharged; os uteri fully dilated. There was a loop of pulseless and flaccid funis in the vagina, and the shoulder presented. The midwife had unfortunately given ergot, having mistaken the presentation. On making an examination with a view of turning, the arm came down. The uterus was so completely under the influence of ergot, that turning was impracticable. Mr. Gordon Brown, one of my colleagues in the charity, kindly gave chloroform. The patient was put deeply under its influence, but it failed to diminish the tonic contraction of the uterus; neither of us could succeed in turning. Decapitation was performed with Ramsbotham's hook, and delivery completed in a few minutes. Mother made a good recovery.

Case 4, December 8th, 1874.—A multipara. The hand and pulsating funis in the vagina; liquor amnii discharged; body of child completely across the brim. Turning, by introducing the hand, easily effected; child delivered alive; and the mother made a good recovery. There was a considerable diminution of the conjugate diameter of the brim caused by the jutting forward of the lumbosacral angle. This patient had twice been delivered by Dr. Barnes, when Physician to the Royal Maternity Charity, for a similar complication as in the present instance; in each case turning was adopted.

Case 5, January 11th, 1875.—Multipara. Both feet, one hand, and the pulsating cord low down in the vagina; the pelvis was very large, and the soft parts exceedingly relaxed. Gentle traction of the feet caused the child to revolve immediately, and it was delivered with great ease. The funis, which was very long, remained external to the vulva all the time; I did not attempt to replace it. Child born alive; the mother made a good recovery.

Case 6, January 29th, 1875.—A multipara. Liquor amnii discharged twenty-four hours; os uteri fully dilated; great obliquity of the uterus to the right side, which was not capable of being rectified by position nor by external pressure. The head presented in the first position, but had not become engaged in the pelvic brim; this was much diminished by an unnatural prominence of the lumbo-sacral angle; the bones of the head were overlapping. Long forceps were tried; the blades of these were not easily adjusted, and they were troublesome to lock, circumstances indicating a corresponding difficulty of delivery. The forceps were disposed to slip, and would have done so had traction been continued. Each blade was removed, and on attempting to reapply them a loop of pulseless funis came down on one side of the sacral promontory. This pulseless condition of the cord, with the coexisting difficulty of a diminished conjugate diameter of the brim, indicated perforation of the head. This was had recourse to, and the cerebral substance and septa of the dura mater being well broken up with the crotchet, it was thought the collapsed head could be easily brought through with long forceps. These were applied again, but still the head would not pass the brim without a hazardous amount of force. The cephalotribe applied at once reduced the base of the skull, and traction with this instrument easily effected delivery. Two of my colleagues, Mr. Gordon Brown and Mr. Taylor, were present, who attended to the administration of chloroform.

Since writing this paper I was requested to meet a gentle-

man in consultation on a case of placenta prævia. He was anxious as regards the occurrence of further hæmorrhage. The patient had been in labour some hours with this her second child. The liquor amnii had been discharged a long time; ergot had been given with a view of restraining hæmorrhage.

On a vaginal examination the greater portion of the placenta could be felt coming down detached posteriorly. No presentation could be felt by ordinary examination. By introducing the hand into the vagina the pulseless cord could be felt, and beyond this the arm and ribs of the child lying across the os uteri, which was well dilated. From the thoroughly ergotized state of the uterus there was much difficulty in getting the hand sufficiently into the uterus to turn, but after a patient trial this was accomplished and delivery effected. The mother did well.

It will thus be seen that prolapse of the cord is, for the most part, associated with some other complication of labour, and that it behoves us to make observations as to what that complication may be; that the management of the prolapsed cord will in a great measure depend on the kind of complication with which it is connected; that in some complications there is a fair probability of proceeding so as to save the funis from long-continued and dangerous pressure, but that in the more serious class of cases the important function of the cord cannot be preserved.

Dr. Hicks asked Dr. Roper if he had employed cephalic version in transverse presentations with prolapse of the cord. He had given reports of two cases of the kind in his little work 'On Turning,' and others had employed it with ease.

The President inquired whether Dr. Hicks combined the postural method; he had experienced great difficulty in getting the prolapsed cord returned.

Dr. Hicks, in answer to the President, said that he had not found great difficulty in getting up the cord; the head is pressed up, and cord carried up by the same hand as quickly as possible, and then the outside hand presses down the head into

the os. It is then carried towards the breech, which is thereby raised to the fundus, and after a few pains have occurred the internal hand is carefully withdrawn, the head being kept in the os by the external hand. In prolapse with head presentation the same idea is carried out, only the pressing the breech to fundus is not needed as in transverse presentation.

Dr. Brunton said he did not think that Dr. Roper had given sufficient significance to the postural method, and that he had passed it over too slightly. For his part he had found it the most satisfactory of all methods of treatment of prolapse of the cord. He had read a paper some years ago to the Medical Society of London, since published in the 'Obstetrical Journal of Great Britain and Ireland,' on the subject, and showed that the postural method was applicable in all cases. He preferred it to all other modes of treatment, and would continue to use it. In reply to Dr. Braxton Hicks's method, he doubted if Dr. Hicks, with his nimble fingers, could reduce by the bipolar method such a prolapse as was sometimes to be met, viz. a long loop of cord protruding from the vagina. By the postural method this could be easily done.

Dr. Roper thought the difficulty was not in restoring the position of the cord, but in keeping it there. He had not found the uterus draw it up as spoken of; but generally found that the next pain forced it down again.

Dr. Edis remarked that the genu-pectoral, or knee-shoulder, and not the knee-elbow, position was the correct one. It made considerable alteration in the axis of the pelvis, depending upon which position was enforced.

Dr. Roper would like to know the *rationale* of the postural position. Gravitation would not come into force, he thought.

The President referred to a paper Dr. Thomas, of New York, had read upon the subject many years ago.

NOTE ON A DISEASED PLACENTA.

By Lawson Tait, F.R.C.S. Ed. and Eng.,

SURGEON TO THE BIRMINGHAM HOSPITAL FOR WOMEN, ETC., CONSULTING SURGEON TO THE WEST BROMWICH HOSPITAL, ETC. ETC.

Mrs. A—, æt. 29, was married in 1870, and became pregnant immediately. Three months after she had a miscarriage.

She became pregnant again soon, and went to the full time with a male child. Just before the birth a severe attack of convulsions set in necessitating the use of chloroform and forceps by two practitioners at Brighton. After this labour there was no flooding. The child died a few days after birth, of convulsions.

The next pregnancy ended by miscarriage at between the sixth and seven months, accompanied by some flooding.

In three months she became again pregnant, and a girl was born about six weeks before the full time, and before the birth there was very serious flooding, which ceased when the placenta came away. The child was very small and poorly nourished, yet it lived, and has grown into a child of average size. Fifteen months after the birth of this child she became pregnant again, and came under my care when she was about six months advanced. From the history I felt certain that the miscarriages were due to the repeated occurrence of disease of the placenta, and that the hæmorrhage had occurred from its premature separation.

With this view I put her on liberal doses of chlorate of potash and small doses, one thirtieth of a grain, of mercury perchloride, with the result that she went to the full time.

The labour was completed almost by two pains, the whole ovum being expelled into a commode. The child

was fully developed and well grown. About half an hour after the labour she had very serious hæmorrhage, which was immediately and effectively controlled by liberal intra-uterine injection of equal parts of vinegar and water. She had a rapid and perfect convalescence.

The chief interest in this case lies in the appearances of the placenta. It was fully examined within three hours after delivery and without the assistance of any chemical process, so that the appearances figured and described cannot in any way be referred to alterations the result of treatment. The placenta was of a size somewhat disproportionately small to the size of the child, and fully two thirds of its surface had undergone a peculiar change. The part affected was not interrupted by any healthy cotyledons, and was shaped like a crescent moon, its long arms extending round the healthy portion, like

"The old moon in the new moon's arms."

It was of a dirty yellow colour, firm and fleshy to the touch, in this respect contrasting markedly with the contiguous healthy portion, as it also did by its greatly diminished thickness.

The only description of this form of disease of the placenta which I have found is given by Cruveilhier, in his 'Anatomie Pathologique,' tome i, liv. xvi, "Maladies du Placenta."

The name he gives to it is *atrophy*, and I think it very applicable to the naked-eye appearances, and also partly to those displayed by microscopic examination.

His description is so vivid that I give it in full:

"It may be general or partial; partial atrophy may occupy a third, two thirds, or three fourths of the placenta; it may invade here and there some cotyledons, leaving intact those intermediate. The consequence of that atrophy is the withering or even the death of the fœtus. Its characters are, a considerable thinning, a kind of drying, the tissue becoming firm, often granular, also tubercular, discoloured, or rather of a whitish-yellow colour.

"In a word, the placenta becomes reduced to its fibrous

basis. It is this condition which has been described by authors as scirrhus or tubercular placenta. I regard this alteration as the result of a separation of the placenta, or of a defect in the communication, whatever be its cause, between the uterus and the placenta at the seat of the atrophy.

"A small number of unaffected cotyledons may suffice to maintain life when the atrophy of the different parts of the placenta has been successive."

M. Cruveilhier further gives a plate of such a placenta when the child had been dead about fifteen days; and which, except that it is partly affected by apoplectic effusion, may be taken to represent very well the naked-eye appearance of the placenta I have described, at least as far as the colour and thinning of the tissue are concerned.

I injected this placenta from the vein with a ten-per-cent. solution of Seitel's Berlin blue in strong size, under a pressure of 300 millimètres. Usually a placenta can be fully injected with a pressure of 80 millimètres. There was no extravasation or rupture, as the placental capillaries, if perfectly fresh, can stand as much as 500 millimètres without injury.

The healthy part of the placenta became readily distended, and on section it was found to be completely occupied, whilst in the diseased part only a few vessels of small calibre were found traversing it, visible to the naked eye; enough only as one might say to save it from absolute death.

The injected tissue was normal, but the uninjected was very firm, fleshy, and much more resistant to the knife than the former.

I cut a piece out so as to embrace the line of division between the healthy and diseased parts of the organ, placed it in my freezing machine, and made sections across the plane of division. These sections were stained with carmine, logwood, litmus, silver lactate, iodine, and alkamine. The two latter gave negative results, so that

the conclusion is that there was no lardaceous change in the coats of the vessels, and no fatty degeneration.

Lardaceous change in the placenta has been stated by a few to occur, but no very trustworthy report of it has come under my notice, and I am compelled to state that I have as yet seen no indication of fatty degeneration in any of the many placentæ which I have examined, and where I have expected to find it. I must here say something of what I have observed of the anatomy of the normal placenta before I speak of the changes which I noticed in the instance just detailed, for otherwise I should not be able to render intelligible what seems to me to be the process of change. The umbilical vessels enter the placenta through closely fitting fibrous rings derived from the tissue of the chorion; and in a series of special researches for the purpose* I failed to find any continuity between the canalicular tissue of the cord and the tissue of the placenta. It is more than probable, however, that some of that tissue accompanies each vessel through the canals, formed by a division into two layers of the outer layer of the placental part of the chorion, which contain the major branches of all the umbilical vessels. At any rate, as soon as these branches pass into the loose tissue of the placenta it is certain that a mass of nucleated canalicular tissue appears round the vessel forming a district for it, exactly as is seen in the cord (Pl. IV, fig. 6). It is almost certain, therefore, that some of this tissue must pass through the rings in the chorion along with each vessel, as it would not be easy to understand how the nutrition of the vessels could otherwise be carried on, for there exists no capillary arrangement here as there is at the omphalic extremity of the cord.

When the vessels are so arranged that an artery accompanies a vein, or, as it more generally is, when two arteries accompany a vein (Pl. IV, fig. 6), the district will be found common to them. At least, as it would be very

* Royal Society, June 17th, 1875, "Note on the Anatomy of the Umbilical Cord."

difficult to apply Recklinghausen's method of injection on so small a scale, we must suppose that the district is common to the vessels enclosed in it, though this may not indeed be the fact. As the vessels break up into capillaries this nucleated canalicular tissue splits into two layers, the outer forming the capsule of the villus, the other forming the nucleated tissue of the villus proper, in which the capillaries lie imbedded (Pl. IV, fig. 5). This outer layer has been stated by Goodsir and Reid to be the maternal part of the placenta, derived from and continuous with the lining membrane of the vascular system of the mother; but this view cannot be reconciled with many of the facts of the anatomy of the placenta, and is completely at variance with the results of my own investigations.

That the tissue surrounding the vessels is canalicular and absolutely analogous to the tissue of bone, save that it has no interstitial deposit of salts, I infer from its absolute similarity to the proper tissue of the cord, which is nothing more than three nutrient tonics for the vessels. This must also be the function of the canalicular tissue in the placenta.

Further (loc. cit.), I have seen reason to believe that the muscular tissue of the arteries of the cord is only a modification in form and arrangement of the elements of this same tissue, the nuclei of the two tissues being identical (Pl. V, fig. 7). This view I propose extending to the placenta.

Indeed, there is less difficulty in believing this than that the increase in growth of the muscular tissue should be due to contributions from a purely external source, the only other, some element of the blood. This harmonises also with the fact that the increase in thickness of the arterial coat is, or at least seems to be, exogenous.

In the arteries of the cord and placenta the fibres are arranged on bands having a double spiral direction, and these are, so far as I can ascertain, symmetrically affected in the placenta I am now discussing.

On examining a section of this placenta with a low

power, the most striking difference was to be noticed between the portion where the injection had entered freely and that where it had done so only to a very limited extent.

In the former the details of structure were exactly as I have already given.

The arteries and veins were all distended uniformly with injection, as were also the capillaries (Pl. IV, figs. 5 and 6). In the diseased part the villi had become greatly shrunken, diminished both in length and diameter, and in some parts they seemed almost to have disappeared. At the border line between the diseased and healthy parts some villi were to be seen which were irregular in outline, thick at one place and contracted at another, with corresponding differences in the diameter of the injection (Pl. II, fig. 1). Occasionally the injection could be seen to fine off to a point as if the tube had become occluded by contraction.

The arteries were found to have their coats greatly thickened, and their calibre very greatly diminished; and in the majority of instances, especially of the small arteries, the tubes were entirely closed. The thickening of the muscular coat seemed to be more marked in the smaller arteries than in the larger. Sometimes in a small artery only a minute speck of injection could be seen. The coats of the veins seemed entirely unaffected, and they were distended with injection just as if the whole structure of the placenta were normal (Pl. II, fig. 2). Careful examination will satisfy any one that there is a thickening of the muscular coat, due to a real addition to the muscular fibre, as well as an apparent increase of thickening, due to contraction of the calibre of the tube. This is well seen by comparing two sections of vessels with districts of similar area, one from the normal part of the placenta, the part where the arterial coats are thickened (as in Pl. IV, fig. 6, and Pl. V, fig. 7). The thickened coats are uniformly composed of elongated, nucleated, fusiform cells, and the canalicular tissue of the surrounding districts differs in no particular from the

same tissue in the normal placenta (Pl. V, fig. 8). As the nuclei are all perfect, I conclude that the function of the tissue was in perfect action; and from the evident increase in their numbers, it seems to me that an increased demand for their action had been made, necessitating an increase in the supply of them.

The apparent increase in number of the nuclei occupying the district of canalicular tissue is also in favour of my view of its relations to the muscular coat of the blood-vessels, this increase in number being more apparent close to the vessels.

The change, therefore, in this placenta seems to have had for its essential object the obstruction of the passage of the blood through the greater part of the organ by a complete closure of the canals of the greater number of the small arteries, and a great diminution of the canals of others, and this has been effected by the action of a wholly normal process,—muscular hypertrophy, the result of the required exercise of increased muscular effort. Accompanying this is the removal of the nucleated tissue of the villi and their capsules, a process which must follow as a result of their disuse.

There arises here at once the question, Which of these is the first step in the process? and though it is not an easy one to answer, still I think a solution is possible.

If we bear in mind John Reid's experiment of putting a ligature on the trachea, we remember that at the last stage the blood pressure in the arteries falls greatly by reason that the blood is hindered passing through the pulmonary capillaries by contraction, as we now know, of the small branches of the pulmonary artery. Capillary hæmorrhage does not uniformly occur, as it would inevitably do if the obstruction lay on the distal instead of the proximal side of the capillaries. Supposing this experiment performed on a bronchiole with the possibility of the animal surviving the experiment, we should probably find that the contraction of the arterioles would become persistent, the capillaries would disappear, the lung would

atrophy. It would be interesting to know if this arterial thickening does actually take place in the cases of pulmonary collapse described by Gairdner.

Here unquestionably the closure and hypertrophy of the arterial tube would be the first step, and the removal of the distal structure, in obedience to the usual law of the atrophy of unused organs, would inevitably follow.

Bearing in mind the function of the placenta as a lung, we see the same process in this case. The umbilical arteries take the place of the pulmonary artery, and the umbilical vein that of the pulmonary vein. In the event of it becoming impossible that the fœtal blood should get oxidized in part of the placenta, the stop-cocks of the small branches of the umbilical arteries come into action to hinder the passage of blood through useless and dangerous channels. As the condition may become chronic in the placenta, atrophy and disappearance of the villi follow as a matter of course. In fact, the process of hypertrophy of the walls and closure of the canals of these branches is exactly what takes place in the main trunks in the neighbourhood of the bladder and umbilicus after the child is born. The immediate cause of this closure of the arterioles must be due, of course, to non-oxidation of the fœtal blood passing through the placenta. That again must be due to some change in the maternal mechanism. What that was in the present case it is impossible to guess, but the patient's history, and the history of many other such cases, shows that there is a specific tendency to such change in the uterine surface, that the interchange between mother and child through the placenta, whatever it be, becomes more or less restricted or completely interrupted.

The disease then is primarily in the mother and not in the placenta, the change in the latter being positively a protective effort to avert risk to the child.

These considerations make me greatly doubt the probability of the occurrence of what has been called fatty degeneration of the placenta. The appearances which

have been figured as such by Dr. Barnes I have often seen, but they are not due to fat, but to commencing or advanced molecular disintegration occurring after the separation of the organ. I have never found them in a perfectly fresh placenta.

It is impossible to regard the facts detailed above without being struck at once, as I was, by the support they give to the conclusions arrived at by Dr. George Johnson from his researches in the kidney.

Between the two organs there are such physiological differences as account for the variations in detail of what we must regard as the same process occurring in the two organs; yet there are also such strong physiological analogies between them as to show that the probable origin and course of this muscular hypertrophy is the same in both.

First of all, whilst it is seen to be a matter of fact that this change may occur in its extremest form over a limited area of the placenta, it is difficult to imagine a set of circumstances which would exhibit the same degree of change in the kidney. Long before it could be reached death would be inevitable, therefore it is not surprising that while the thickening of the coats of the renal arterioles is sufficiently evident in fatal cases even where the kidneys have not contracted very markedly, it is never seen to the extent evident in my placenta.

The exact function of the placenta is yet unsolved, but we are at least certain that it is an excretory organ, its power of excretion being dependent on the existence of a certain relation of its villi to the maternal circulation, whatever that relation may be. The kidney also is an excretory organ, its function depending on a certain relation between the state of its epithelium and the quality of its blood supply. On the occurrence of a disturbance of these conditions in the kidney, a diminution of the blood supply to the organ would be the first attempt at rectification.

In the event of the disturbance becoming chronic, the

contraction of the arterioles would be constant, and this would necessitate an increase of their muscular tissue.

If the disturbance arose in the blood, the conditions in the kidney would be invariably symmetrical, as it is in contracted kidney.

If it arose from primary alteration in the epithelium, it might be expected to be at least sometimes unsymmetrical.

In the placenta the shifting juices of the mother stand in place of the epithelium, and as the changes are not symmetrical, I presume that the primary alteration takes place in the structures which convey the maternal juices.

If it were primarily in the child we should expect that the affection of the fœtal tufts would be symmetrical, but it is not so.

Upon this important point, however, it is certain that more evidence must be had, but I think that the presumption is in favour of my view that the primary change is external to the fœtal tissues.

There is only one pathological condition of the kidney with which I am practically familiar: that found in cases of puerperal eclampsia. In that disease it is certain that the primary affection is a blood change, for several reasons. For instance, no change is found in the kidney save extreme hyperæmia and a choking of the tubules with epithelium, as if there had been a sudden eruption of some morbific material in the blood which the kidneys had made a hurried and ineffectual effort to excrete. The disease is completely arrested by chloroform; and finally, chronic Bright's disease, even in its advanced stage, occurring coincidently with pregnancy, does not produce the phenomena of puerperal eclampsia. This I have frequently had occasion to note, but more especially in a case which I saw recently in consultation with Dr. Call Weddell, in which the patient lived through the later months of pregnancy with all her tissues greatly œdematous, and her urine loaded with albumen, yet she died quietly a few hours after labour without the occurrence of

a single convulsion. She seemed to have her life prolonged just as we more frequently see in cases of phthisis where the pregnant woman is apparently in hourly risk of death, yet lives for months till labour is over, then sinks in a few hours.

The renal epithelium being so altered by this sudden eruption in the blood, the normal condition of excretion is disturbed, and the disease beginning in puerperal eclampsia often ends in chronic Bright's disease, and after death contracted kidney is found. One such case I can remember very well where I happen to know that until after the occurrence of several convulsions no albumen was found in the urine, but after it did appear it was never absent, and the patient sank in a few months with well-marked contracted kidneys.

This leads me to suspect that the primary change in the majority of these cases must be looked for in the blood, leading to destruction of epithelium. Whether this be so or not, from this point my conclusions are the same as those of Dr. Johnson, which I give in his own words (Medico-Chirurgical Society 'Transactions,' vol. lxi, p. 51). "In proportion to the destruction to the renal gland cells, and the consequent diminution of the secretory power of the kidney, there is less demand for blood to be acted upon by the gland; the small arteries consequently contract upon their contents, so as to maintain the balance between the blood supply and the diminished secretory action of the kidney. The continuous overaction of the small arteries in antagonism to the heart results in hypertrophy of their muscular walls."

I have to acknowledge my obligation to Dr. Johnson for the opportunity of examining perfectly fresh specimens of contracted kidneys. I am in a position to substantiate his description completely.

The only difference it seems to me, evinced by the process in the kidney and that in the placenta, is that in the former the change is effected in the epithelium by a previous change in the blood; whilst in the placenta the

primary change takes place in the maternal mechanism, as might be expected from the fact that the fœtus has a dependent existence.

Further, it is probable, though not in evidence, that no further change takes place in the tissues of the fœtus such as is seen in the tissues of the independent individual in whom the kidney is affected, where the muscular coats of the arterioles of the skin, brain, &c., become also hypertrophied. The reason of this is evidently that the diminished circulation through the kidney involves the retention in the blood of matters detrimental to the nutrition of other organs, and they resist their entrance.

From the arrangement of the fœtal circulation, which allows of the constant circulation everywhere of mixed blood, a constant ratio in the area for oxidation is not of essential importance so long as there is just enough to supply oxygen sufficient for the systemic changes, at a probably increased rate of transit over the diminished area. This is proved by the extraordinary variations there are in the relations between the size of the placenta and that of the child; and by the fact that in the case which is the basis of this note, there was probably an area of normal placenta of not more than a fifth of the usual size, yet the child was well developed and alive. Again, in animals where the foramen ovale does not close for some days after birth, as in the cat, the young may be kept immersed in water if its temperature be not below blood heat, for a long time, as much as half an hour, before death takes place.

This leads me, in conclusion, to mention a matter which is now closely occupying my attention, that there is reason to believe that the placenta is less exclusively engaged in the nutrition of the child than is generally supposed; that it is probably little more than a lung; or that, at least, it has an important auxiliary for the nutrition of the fœtus in the liquor amnii.

Certainly the facts of the case I have narrated point in

this direction, for had that child been nourished by the placenta exclusively, I do not think its development could have been so complete with so restricted a supply of pabulum. Moreover, when the fœtal surface of the amnion was treated with a ·001 per cent. solution of silver lactate, its structure was seen to be perfectly normal.

Whilst engaged in studying the placenta from another point of inquiry, I noticed in a paper by Dr. Fränkel, of Breslau, that one of the plates depicted the arterial thickening which I had observed. His paper is on syphilitic disease of the placenta, and without here going into so wide a question, I wish to state my conviction that he has not established the existence of such a disease.

There is a prevailing fashion, especially in Germany, to find new syphilitic lesions, and if we accept all such as are described we must conclude that syphilis is a more common exanthem than measles. Dr. Fränkel's method of inquiry is very simple. He takes it for granted that Wagner has established that the so-called congenital osteo-chondritis is necessarily syphilitic. He further accepts what we all know to be the fact, that women who are syphilitic or married to syphilitic husbands often miscarry.

Then he merely argues that all women who have miscarried two or three times, and in whose placentæ there are changes, are syphilitic, and describes the changes under the same category. At least, such is the conclusion I draw from many of his cases, for he does not give any good reason whatever for his presumption that they were syphilitic, and sometimes has to confess that there was not the least trace of the disease. Such converse arguments are easy, but not convincing.

There is perhaps no better test of a lesion being syphilitic than that it yields to anti-syphilitic treatment.

I do not know that any one claims for chlorate of potash any influence over syphilis, but from very abundant experience I know that it is an absolute specific against

habitual miscarriage in a very large number of cases, whilst in the truly syphilitic cases I have not found it to have any power whatever.

Fränkel depicts the appearances I have described, but he misrepresents their meaning in a curious way. The canalicular nucleated tissue, of perfectly normal character, he describes as "stärk Granulationszellwucherung," and states exactly as I have stated, "Um die Gefäss herum ist die zellwucherung am stärksten." The closure of the vessels he describes as "leichter fibröser Degeneration des Stroma's," whereas, if he had prepared his specimens from fresh tissue instead of from what had been chemically altered by hardening reagents, he would have seen that there was nothing fibrous about the tissue, and no trace of degeneration, whilst the simplest form of reasoning might have led him to doubt the probability of *degeneration* being accompanied by actual, not merely apparent, increase of tissue, and having the most active cell growth in its immediate neighbourhood.

Whatever be the origin of the changes Fränkel describes in the villi—and they closely resemble in some points those which I have described—the changes in the arteries are essentially conservative, not due to any kind of degeneration, but to actual hypertrophy of normal tissue. They are essentially therefore secondary, and analogous to those of Bright's disease, as I have already said.

EXPLANATION OF PLATES II, III, IV, V,

Illustrating Mr. Lawson Tait's paper on a Diseased Placenta.

Plate II.

Diseased placenta. Frozen section. Hæmatoxylin.

Fig. 1.—Capillaries into which the injection had partially penetrated. × 260 : 5.

Fig. 2.—V, Vein, transverse section. A, A, A, Arteries, transverse section. × 120 : 4.

Plate III.

Kidney from case of Bright's disease, supplied to me by Dr. George Johnston, F.R.S. Hæmatoxylin. Frozen sections.

Fig. 3.—A, Artery, with hypertrophied muscular coat. × 260 : 5.

Fig. 4.—B, B, Oblique sections of arteries. C, Transverse section of arteries. D, Transverse section of vein. Frozen. × 120 : 4.

Plate IV.

Fig. 5.—Villous and nucleated capsule of normal placenta. × 90 : 4.

Fig. 6.—District of canalicular tissues surrounding vessels in normal placenta. Frozen. Transverse section.
A, A, Arteries. V, Vein. C, Villus, with capillaries.
× 90 : 4.

Plate V.

Fig. 7.—District of canalicular tissue surrounding vessels in diseased placenta. Transverse section. × 90 : 4.

Fig. 8.—Nuclei of canalicular tissue surrounding blood-vessels in diseased placenta. × 1000 : 5.

J. T. del. March 21st 1875.
Dr Westmacott dirext

W. West & Co. lith.

IV.

L. T. delt. June 22nd 1875.

V

L. T delt. June 24th 1875.
Dr Westmacott dirext.

W. West & Co lith.

J. Godart del.t Dr Westmacott direx.t

W West & Co. ch. lith.

spinal cord, the canal itself being lined by a thin fibrous membrane. The mother had had five children; three were living, healthy and intelligent, with no deformity; the other two died when young. She attributed this monstrosity to a fright she had when two months advanced, and stated that she was pursued by a bull. Mr. Barber received an urgent message from a midwife to attend and assist her. On arrival he found she was unable to make out the presentation, but what had frightened her the most was the enormous quantity of liquor amnii which had gushed out when she ruptured the membranes.

On making an examination, at first he thought it was the breech presenting, as the open spinal canal which he described closely simulated the anal aperture, but near this could be felt an ear attached to a rounded mass which he could grasp with the hand, but which in nowise resembled the fœtal head. Judging it would be best to bring this down he accordingly made traction upon it, and on getting it external found it was an anencephalic fœtus. There was no further difficulty, and the rest of the labour was easily accomplished, the woman making a good recovery. The chief points of interest in connection with the case were, firstly, the difficulty experienced in the diagnosis, and, secondly, the enormous quantity of liquor amnii, which was sufficient to deluge the floor and drain into the room below, much to the alarm of the midwife and the other women present. The woman was extremely large before labour, and suspected she would have twins.

Dr. WILTSHIRE said the specimen appeared to be one of considerable rarity as well as of great practical importance. It answered mainly to the description given by Geoffroy St. Hilaire, of the variety of anencephalic fœtuses called derencephalic; and as specimens of this were extremely rare, he hoped Mr. Barber would present this one to the Society. The aperture in the upper part of the spinal canal had very naturally caused embarrassment in the diagnosis of the presentation. He praised the practical manner in which Mr. Barber had dealt with the case.

In reply to a question from Dr. Cleveland,

Mr. Barber stated that no fœtal movements were noticed during pregnancy.

The President inquired if any dissection had been made of the cranial and spinal nerves, with the view of ascertaining how they terminated in the skull and vertebral canal respectively: Some years ago he had seen an anencephalic child born alive, and which had breathed and sucked for some days. The malformation was less extreme than in the specimen before the Society, for the spinal cord was present, but there was no roof to the skull, and the brain substance was represented by a flattened mass of cystic structure, into which the cerebral substance had degenerated, or which had been developed there, instead of the brain. He had made a careful dissection of the cranial nerves in that case, and found that inside the skull, instead of terminating in loops, the nerve filaments gradually merged, and were lost in the cellular tissue which lay between the small cysts composing the mass which represented the brain in the base of the cranium. In the preparation thereon was an opportunity of ascertaining the termination of the spinal nerves in the spinal canal, when the cord itself was absent.*

Dr. Goodson showed a drawing of the breasts of a girl, æt. 18, who had been in St. Bartholomew's Hospital, under the care of Dr. Southey, for chorea. It was ascertained that she was seven months advanced in pregnancy, and the point of interest was the manner in which the pigment had been deposited around the nipple. A portion of the skin on either breast to the inner side of the nipple was entirely free from discoloration, while the pigmentation of the remaining portion was unusually dark, and crescentic in form, so that the appearance presented was very remarkable. The girl herself was present for the Fellows to inspect.

Dr. Barnes said the case was extremely interesting. The symmetrical defect of pigment might be explained by like areas of nerve-distribution. In all probability the altered nervous condition of pregnancy had much to do with pigmentation. He wished to refer to the complication of chorea in this case. He learned from Dr. Godson that the subject had had attacks of chorea before pregnancy. This was an illustration of views he had

* Dr. E. J. W. Hicks kindly made a dissection of the kind indicated by the President, and found that the central ends of the nerves he examined terminated by losing themselves in the cellular tissue of the parts.—Ed.

expressed in his memoir "On Chorea in Pregnancy," submitted to the Society some years ago, and in his Lumleian Lectures before the College of Physicians. Under the exalted nervous tension wrought by pregnancy, aided probably by some change in the constitution of the blood, any pre-existing disposition to chorea or other nervous disorder was apt to be evoked. Thus pregnancy was an exciting cause of chorea.

The PRESIDENT inquired whether there was any history of injury from a scald or burn. The case presented the usual characters of some skin-eruptions, in that it was symmetrical.

Dr. WILTSHIRE thought the case peculiar and interesting, and worth preserving in the 'Transactions' of the Society, if Dr. Godson would give the coloured drawing.

Dr. GODSON stated that there was no hereditary tendency to such a condition. The mother had two nipples on one breast.

Dr. HAYES thought the fact of chorea and pregnancy existing together was one of much interest. He had seen one hundred cases of chorea, and had noted the very marked relation between chorea and rheumatism.

CASE OF EPITHELIOMA OF THE CERVIX UTERI; PREGNANCY; PARTURITION; EXTRACTION OF A LIVING CHILD; DEATH OF THE MOTHER TWO WEEKS AFTER DELIVERY, FROM PYÆMIA.

By Dr. ARTHUR W. EDIS, M.D.,

ASSISTANT OBSTETRIC PHYSICIAN, MIDDLESEX HOSPITAL.

(The notes of the case were taken by W. M. BEAUMONT, the Resident Obstetric Assistant.)

THE patient, A. H—, æt. 33, was sent by Dr. March, of New Wandsworth, as an out-patient at the Middlesex Hospital, on June 26th, 1875.

She then complained of losing blood on and off for the last few months, clots being passed occasionally, but unattended by pain, except at times in the lower back and left iliac region.

On examination the uterus was found to be enlarged nearly to the ensiform cartilage. The fœtus lying in the first obstetric position, with the back towards the mother's

left side, the breech close up to the fundus, was clearly determined, and fœtal movements detected. Utero-gestation had apparently advanced to about the 7½th month.

On examining *per vaginam*, the cervix uteri was found to be in a state of advanced epitheliomatous degeneration, the disease creeping on to the posterior vaginal wall and left cul de sac. It bled readily upon touch, and there was a sanious muco-sanguinolent discharge. The fœtal head could be detected presenting at the pelvic brim.

The nature of the disease and the difficulty likely to occur at the time of labour having been explained to the patient, she was desired to report herself again in a few weeks' time.

The question of Cæsarian section or the induction of premature labour was contemplated; but as there was no vacancy in the hospital at the time, and one portion of the cervix seemed to be unimplicated in the diseased process, it was a question whether dilatation of the cervix could not be effected and delivery accomplished.

State on Admission.—Aug. 7th, 1875.—Patient brought here at 1 a.m., stating that she was in labour. On examination the os was found to be encircled for three fourths of its circumference by epitheliomatous growths, partially involving the vagina in one spot only. Two fingers passed easily within the os uteri, and a fœtal head was detected presenting. Bearing-down pains, not very severe, occurred every quarter of an hour. Membranes not ruptured. Patient is thin but florid. The abdomen is uniformly enlarged in a globular manner up to within two inches of the ensiform cartilage. The tumour is smooth, firm, and resisting. Fœtus felt lying in the first position, and fœtal heart heard at usual spot. Skin cool; pulse 92; temp. 98·3°. Put to bed, and suppos. morph. gr. ⅓ administered, but patient did not sleep much in consequence of pain. Tongue moist and clean.

History.—Patient has always been regular excepting during pregnancy and lactation. Married at 27; quite

well up to that time; first child born ten months afterwards; still living. It was a natural labour as far as she knows. Her second child was born about three years ago, that is, two and a half years after the first; it was also a natural labour, and the child is still living. Suckled the child for a year and nine months, and weaned it last Christmas. She believes her last menstrual period occurred a little before that time, but she has had floodings since on several occasions. About January last had several attacks of flooding, also again in February. Pain in the left side came on like "stitch in the side," never lancinating in character. She has not lost flesh, and there is no family history of cancer. Has had a discharge from vagina since Christmas; never noticed that it was fœtid. She had rheumatic fever nine years ago.

9 p.m.—Pulse 96; temp. 99·2°. Skin hot and rather dry. Slight pains. Two medium-sized Barnes's bags inserted and distended within the os uteri. Chloral hydrate, gr. xx administered.

8th.—1 a.m.—Sick twice since 9 p.m. Four ounces more water injected within Barnes's bags. Pains now every quarter of an hour. Apparently stronger; she dozes between them. Repeat chloral hydrate. Pulse 100; temp. 102·2°.

9 a.m.—Pulse 86; temp. 88·8°. Pains now every five minutes. The anterior bag protruding out of vagina and removed. The other bag distended. Patient has dozed through the night.

10 a.m.—Os fully dilated and head presenting. The healthy tissue had stretched considerably, whilst the epitheliomatous mass filled in the concavity of the sacrum. Urine drawn off by catheter, and chloroform administered. Long forceps applied, and after a little delay a living female child extracted, weighing six pounds and a half. During delivery the sides of the vagina were cut with scissors to admit of the passage of the head, but notwithstanding this, the perinæum ruptured as far as the sphincter muscle. Some slight hæmorrhage afterwards

was easily controlled by ice, the uterus contracting well. Sutures were put in the perinæum.

12.30 p.m.—Pulse 128; temp. 99°. Patient says she is comfortable.

3 p.m.—Pulse 120; temp. 99°; tongue white. No hæmorrhage.

6 p.m.—Pulse 116; temp. 102·4°. Has been dozing through the day. Vagina washed out with Lotio Acidi Carbolici; discharge offensive. Pil. Quiniæ (gr. j) ℂ opii (gr. ¼) statim. Urine drawn off by catheter (℥xv).

10 p.m.—Pulse 120; temp. 101·8°. Five ounces of urine drawn off; tongue clean. No hæmorrhage.

11.15 p.m.—Pulse 138; temp. 101·8°. Feels sore; otherwise comfortable. Repeat pill.

9th.—9 a.m.—Passed a very comfortable night, though not sleeping much. Pill was repeated at 4 a.m. Took nourishment well through the night. Pulse 116; temp. 100°. About fifteen ounces of urine drawn off.

10 a.m.—Pulse 120; temp. 101·2°. Vagina washed out. Abdomen tense, tympanitic, and tender. Perinæum appears to be uniting. Binder readjusted. Bowels not open. Repeat pill 4tis horis.

2 p.m.—Pulse 140; temp. 101·2°. Vagina washed out.

5 p.m.—Urine drawn off.

6 p.m.—Vagina washed out. Pain over the pubes. Pulse 126; temp. 101·8°. Bladder empty.

10 p.m.—Pulse 118; temp. 101·4°. Tongue clean and moist. Abdomen tympanitic. Patient has passed wind downwards. Vagina washed out and urine evacuated.

10th.—10 a.m. Passed a very comfortable night. Pulse 120; temp. 101·4°. Milk appearing in both nipples. Urine drawn off. Eats well. Abdomen tender.

2 p.m.—Pulse 128; temp. 101·8°. Passes fæces involuntarily. Turpentine stupes to abdomen. Vagina washed out and urine drawn off.

6 p.m.—Pulse 128; temp. 101·1°. Feels sick. Urine drawn off. Charcoal poultices to genitals. Diarrhœa.

10 p.m.—Pulse 120; temp. 101·8°. Diarrhœa con-

tinues. Enema Amyli cum Tinct. Opii ♏xxv. Pills continued. Pain in knees.

11th.—10 a.m.—Passed a good night. Diarrhœa ceased. Eats well. Breasts not distended. Omit charcoal poultices. Genitals appear to be less tender. Pulse 112; temp. 100·8°. Vagina washed out and urine drawn off.

4.30 p.m.—Temp. 103.4°.

10 p.m.—Pulse 128; temp. 103°. Vagina washed out and urine drawn.

12th.—10 a.m.—Pulse 118; temp. 100·8°. Bowels open twice in the night and three times this morning. Abdomen slightly distended, soft, and resilient. Slight tenderness on pressure over left iliac region.

10 p.m.—Pulse 128; temp. 102·8°. Complains very much of her fæces running away from her. Pain over pubes. Enema Amyli cum Tinct. Opii ♏xxv. Scarcely any discharge from vagina, but very offensive from rectum. Repeat pill statim and in four hours if necessary.

13th.—10 a.m.—Passed a comfortable night. Bowels have acted several times this morning. Breasts distended more with milk. Catheter and vaginal syringe used. No pain in abdomen except directly over pubic bone. Very little discharge, and less offensive. Eats well. Pulse 112; temp. 100·8°. Perinæum seems to be healing.

2 p.m.—Pulse 118; temp. 101·4°. Bowels open twice since 10 a.m. Breasts emptied of milk by baby. Has eaten a good dinner: chicken, custard, &c.

7 p.m.—Bowels have been opened three times since 2 p.m.

10 p.m.—Pulse 128; temp. 101·2°. Feels comfortable. Bowels open once since last note; more solid. Vagina washed out. Still unable to pass water naturally.

14th.—10 a.m.—Did not sleep quite so well last night; says she has pain in her back bone. Bowels open three times in the night. Tongue clean. Eats well. Pulse 124; temp. 102·8°. Discharge much less offensive.

3.30 p.m.—Pulse 156; temp. 103·6°. Feels hot and feverish. Bowels open twice since this morning. A piece

of epithelioma loose in the vagina removed; size one inch by an inch and a half. Uterus washed out by catheter; very offensive.

10 p.m.—Bowels open three times since last note; patient complains continually. Repeat enema. Felt sick an hour ago. Had oysters since. Bad taste in her mouth. Uterus to be washed out once daily and vagina twice. Pulse 132; temp. 102°.

15th.—11 a.m.—Passed a comfortable night, bowels acting three times. Feels feverish. Quiniæ gr. x. Pulse 152; temp. 105°. Vagina washed out at 9 a.m.

11.30 a.m.—Temp. 105°; pulse 140. Uterus washed out.

12.30 p.m.—Temp. 103°; pulse 132; resp. 32.

2 p.m.—Pulse 128; temp. 104°. Did not eat much dinner. Bowels open twice since 10 a.m. Semisolid.

4.30 p.m.—Pulse 128; temp. 103·8°. Bowels open twice since 2 p.m. Has just taken three oysters.

6.30 p.m.—Pulse 128; temp. 103·8°. Bowels open twice since last note. Had a headache after quinine, which has just passed off. Bowels injected with warm water, which returned immediately.

8 p.m.—Temp. 130°. Bowels not been opened since 6.30. Vagina washed out; discharge very offensive. Says she is sleepy. Catheter has to be used. Not eaten much, but drinks.

16th.—Tongue moist. Dozed through the night; delirious. Pulse 134; temp. 104°. Bowels open six times since last note. Urine passed without catheter in the night. Vagina washed out. Sutures removed from perinæum. No further mass of sloughing tissue detected. Bed-rest.

3 p.m.—Has slept about an hour. Bowels open twice. Pulse 140; temp. 104·6°. Says she feels better for being propped up in bed. Uterus washed out.

7 p.m.—Sat up for about an hour in a chair. Pulse 140; temp. 105°. Vagina washed out. Injections now used four times per diem.

9.30 p.m.—Pulse 132; temp. 104·2°. Tongue moist

and clean. Bowels open three times since midday. Urine by catheter. Profuse discharge of pus from vagina. Vagina washed out.

17th.—Passed a fair night; no delirium. Bowels open three times during that time. Urine comes away if she coughs. Discharge from vagina less offensive. Sat up in a chair for three quarters of an hour. Pulse 120; temp. 102·6°.

2 p.m.—Pulse 130; temp. 103·4°. Complains of pain in the shoulders and left wrist. Has been sitting up an hour and a half without suffering. Chop dinner. Bowels open once since last note. Catheter used.

10 p.m.—Pulse 124; temp. 104°. Bowels open three times since midday; better in colour, but very offensive. Considerable discharge of purulent-looking matter from vagina. Has eaten better to-day. Sat up for one hour and a half this evening. Catheter. Less thirst.

18th.—Did not sleep very well. Bowels open five times since 10 p.m. last night. Had two pills, as usual, in the night. Pain in left wrist joint, also proximal phalanx of left index and right shoulder. Discharge from vagina profuse. Tongue clean and moist. No delirium last night. Pulse 128; temp. 103·4°.

5 p.m.—Pulse 168. Taken food well. Index finger of left hand inflamed. Extr. Opii., gr j in pil.

6.30 p.m.—Temp. 104·8°. Uterus washed out.

10 p.m.—Pulse 128; temp. 105°; resp. 28. Had a slight rigor at 5.30 p.m., checked by hot brandy. Taken oysters to-night. Very thirsty. Repeat pill. Lemonade.

19th.—Dozed through the night after one grain and a half of opium in pill. Delirium. Catheter had to be used. Bowels not open until 6 a.m. No rigors. Pulse 132; temp. 103·4°; resp. 28. Not so much discharge as yesterday. Tongue clean. Skin dry. Pupils rather contracted.

1.30 p.m.—Pulse 136; temp. 104°. Bowels open twice

since 6 a.m., much less offensive. Has eaten a chop for dinner. Urine acid, sp. gr. 1010; trace of albumen. Uterus washed out. Patient very drowsy.

10 p.m.—Has been dozing through the evening, but now rather restless. Taken nourishment well. Pulse 132; temp 104°. Bowels open twice since 1.30. More discharge; very offensive. Elbows and knees stiff. Picks at bed-clothes. No rigors. She appeared to be less rational. Tongue clean. Pupils slightly contracted. Vagina washed out, as usual, four times, and uterus once per diem. More helpless.

20th.—Pulse 148; temp. 104·5°. Passed a restless night. ℈j of chloral hydrate at 4 a.m. without effect. No rigors. Joints very stiff. Blisters have appeared at the inner sides of the ankles. Bowels open four times in the night. Brandy half an ounce every hour since 4 a.m. Does not speak now. Resp. 36.

5 p.m.—Temp. 104·8°. Calomel six grains.

6 p.m.—Vapour bath for fifteen minutes. Much weaker. Temp. 105°. Bowels open well once.

10 p.m.—Breathing stertorous. Very flushed. Pulse 144; temp. 105·2°. Taken nourishment well. Vagina washed out.

21st.—Died at 7.50 this morning.

	9 a.m.		2 p.m.		9 p.m.	
Day of month.	Pulse.	Temp.	Pulse.	Temp.	Pulse.	Temp.
Aug. 7 . .	92	98·3°	...	...	96	99·2°
„ 8 . .	86	98·8°	120	99°	120	101·8°
„ 9 . .	116	100°	140	101·2°	118	101·4°
„ 10 . .	120	101·4°	128	101·8°	120	101·8°
„ 11 . .	112	100·8°	...	103·4°	128	103°
„ 12 . .	118	100·8°	...	...	128	102·8°
„ 13 . .	112	100·8°	118	101·4°	128	101·2°
„ 14 . .	124	102·8°	156	103·6°	132	102°
„ 15 . .	152	105°	121	104°	...	103°
„ 16 . .	134	104°	140	104·6°	132	104·2°
„ 17 . .	120	102·6°	130	103·4°	124	104°
„ 18 . . .	128	103·4°	168	104·8°	128	105°
„ 19 . .	132	103·4°	130	104°	132	104°
„ 20 . .	148	104·5°	...	104·8°	144	105·2°
„ 21 . .	Died at 7.50 a.m.					

No. 208. Post mortem, August 21st, by Sidney Coupland, M.B., was made seven hours after death. Weather warm, fine. Rigidity slight. Body well nourished.

Thorax.—Lungs somewhat engorged posteriorly; elsewhere pale; but healthy.

Heart full of fluid blood. On the auricular surface of the anterior cusp of the mitral valve close to its margin was a slightly raised patch of vegetations, firm and fleshy, of oval form, and about one quarter of an inch in length. It was fringed by a number of minute bead-like vegetations. There was no corresponding vegetation on the posterior cusp; only a slight vascularity. Aortic valves healthy.

Spleen contained several irregular-shaped masses, firm and fleshy, one half are with irregular margins, and not differing in colour from rest of splenic tissue, but only in consistency, which was much increased; extended across the whole of the outer surface of the organ, a few smaller ones were quite colourless.

Kidneys pale. *Liver* slightly fatty and pale.

Pelvis.—Uterus occupied the whole pelvic cavity, the rectum being displaced to the right; bladder empty. In removal of the uterus, an abscess was laid open on the left side of the vagina. It contained a quantity of inodorous pus. The uterine wall was soft and pale. The cavity of the uterus was large, and there was no distinction between the body and cervix owing to the widely dilated condition of the latter. The interior of the uterus was covered with a thin ashy coloured material, which could in great measure be washed away by a stream of water, leaving only a few adherent shreds behind. At the external os the cervix was broken up into several firm prominences of a slate colour; and a large sloughing surface was exposed chiefly in the posterior cul de sac of the vagina. There was a patch of ulceration of the meatus urinarius.

Heart $8\frac{1}{4}$ oz.; right lung 12 oz.; left lung $10\frac{1}{2}$ oz.; liver 59 oz.; spleen 6 oz.; right kidney 4 oz.; left kidney $4\frac{1}{4}$ oz.

Dr. Coupland examined microscopically a small portion of the cervix uteri, embracing one of the papillary outgrowths that still remained. He found but little evidence of epithelioma remaining, except here and there tubes of cylinder-epithelium dipping into the uterine tissue. There was, however, much inflammatory material, especially near the surface of the growth, which is largely infiltrated with leucocytes. Nearly the whole of the epitheliomatous tissue had sloughed away, leaving a ragged surface as described above.

Dr. Routh thought that Cæsarian section ought to be more often performed in these cases. At any rate, the Cæsarian operation gave the best chance of life to the child; and from the experience he had acquired in Vienna, and from what he gathered generally from other examples, where delivery was permitted to occur *per vias naturales*, the death of the mother was almost certain. As very little choice was left to the mother whichever course was adopted as to life, why not save the child? And he really believed that it would be often better for the mother. Puerperal fever generally carried the mothers away, and fatal sloughing of the cervix almost invariably followed if labour was allowed to take place.

Dr. Barnes could not assent to the conclusion that the Cæsarian section should be resorted to in every case of cancer of the uterus. The cases cited by Dr. Routh occurred in the Vienna Hospital, where many other causes of puerperal mortality were rife. He himself had seen women survive labour complicated with cancer. One woman had been delivered twice. Dr. Meadows assumed that if all the cases were analysed, it would be found that the results would be as in those observed by himself. But this analysis had not been made; and it was not safe to frame a general law upon an assumption. The best plan at present was to consider each case as it came before us on its individual merits. In cases where the disease was of small extent, it might be safer to let delivery take place in the ordinary way. Speaking from memory, and open to correction, he thought Dr. Oldham* had published in the 'Guy's Reports' cases which showed that it was better to take this course. Dr. Oldham's opinion was in favour of not inducing labour, but of letting

* Subsequent reference to Dr. Oldham's memoirs ('Guy's Reports,' vol. vii) strengthens the conclusion in favour of the Cæsarian section in *advanced cancer*, but leaves the question as to the lesser degrees of diseases open.—R. B.

pregnancy go on to term, and then to act according to indications.

Dr. Meadows stated that his experience entirely corroborated that of Dr. Routh, viz. that the risk to the mother in cases of extensive cancerous disease of the cervix was so great that the Cæsarian section was preferable to any other mode of delivery. He had seen four cases of this kind, and all ended fatally to the mother, while only one child was saved; in one craniotomy had been performed, in two premature labour was induced. He believed that the Cæsarian section gave a better chance to both mother and child. Of course he was referring only to cases where the disease was so far advanced that an operation of some kind was necessary for delivery.

The President inquired if Dr. Meadows really meant that he would perform Cæsarian section in *all* cases of labour complicated with malignant disease of the cervix uteri; for if so, he believed this was an opinion which most practitioners would think required some qualification. While not doubting the propriety of this operation in cases where the passages were much obstructed by morbid growth, or where serious laceration of the parts must ensue if delivery was effected through the natural passages, he could not concur in regarding it as necessary in all cases where disease was less advanced, and he was sure the Society would be glad to hear the opinions of men with such practical experience as Dr. Barnes and Dr. Routh.

Dr. Meadows replied that he would not limit his opinion to the extent of the disease, but would perform it in all cases.

Dr. Aveling thought that the operation of Cæsarian section would not be justifiable if only one lip of the uterus were involved.

Dr. Rogers stated that in thirty-five years only two cases of this nature had occurred. The question of Cæsarian section was one of much importance.

Dr. Routh stated that, in the cases he had seen, it was impossible to deliver except by craniotomy.

Dr. Edis explained that he brought forward the present case, not with the view of advocating similar treatment in all cases—each case required to be treated on its own merits—but as an instance of delivery *per vias naturales* where the disease was very extensive. Nature herself took the initiative, and seemed to indicate that delivery was possible; and for this reason Cæsarian section was not resorted to.

NOTE ON THE POST-MORTEM DIAGNOSIS OF A NULLIPAROUS UTERUS.

By Alfred Meadows, M.D. Lond., F.R.C.P.

I am induced by the circumstances connected with a recent trial to bring under the notice of this Society a question of some physiological and forensic interest, and one which, in the instance to which I refer, assumed an importance equal to any that could be submitted to a medical witness; for it is probably not too much to say that upon the issue thus raised depended, to a very great extent, the character of a verdict which should carry with it the question of life or death. Indeed, it must, I think, be admitted, that if an absolute test existed whereby a positive opinion could have been given in the negative in regard to the question whether or not this uterus had borne a child, the whole case for the prosecution would have failed, at least on the first indictment; for as it was well known that the person alleged to have been murdered had borne children, a positive scientific opinion to the contrary in regard to this particular uterus would practically have settled the question which was submitted to the jury. Hence the vast importance of this question, for while every right-minded man shrinks with horror at the thought of an innocent man being condemned to an ignominious and disgraceful death, our highest instincts equally revolt from the idea that an error of judgment or a false opinion, even when honestly arrived at, may be the means of shielding a murderer from the just punishment of his crime. It was with these mingled feelings that I went into the witness-box on the occasion to which I refer, and I must confess that I felt my responsibility considerably lightened by the fact that I could not speak with even an approach to certainty on the question submitted to me.

At the same time, I must own that I felt a craving desire for more accurate knowledge, if not in that particular case, at least in any similar one on this special point. The peculiar circumstances of this case, in regard to the length of time supposed to have elapsed since the death of the person, no doubt complicated matters: but I fear the opinion I gave as to the impossibility of determining with absolute certainty whether in any given case, even the most favorable, a person has or has not borne a child must be endorsed by this Society. I shall be glad to be corrected on this point; and it is in order to obtain, if possible, more information than I at present possess on this very interesting question that I venture to bring the subject forward to-night. I know that differences of opinion exist, but I do not know how far there is a concurrence of testimony on one particular point upon which I chiefly relied in the case cited.

The uterus which was submitted for my opinion had the following characters:—Its cavity measured exactly two and half inches in length, its transverse measurement at the fundus was about one and one third of an inch; the shape and dimensions of the os I could not determine, as they had been damaged by post-mortem examination. The walls of the organ were extremely thin, the anterior being rather under, and the posterior rather over, one quarter of an inch in thickness; the lining of the cavity was smooth throughout; the shape of the walls on the internal aspect of the body of the organ, and especially of the fundus, was *distinctly* CONVEX, and it was, of course, chiefly on this ground, as well as, though to a less extent, on account of the thinness of the walls, that I relied in giving the very guarded, not to say negative, opinion which I expressed, and which was to this effect:

First, that as a general rule no absolutely certain opinion can be given by post-mortem examination only, even under the most favorable circumstances, on the question whether or not a woman has borne a child.

Secondly, that on the question of probability some reli-

ance may be placed on the internal appearance of the uterine walls, especially with reference to their convexity.

Thirdly, that so far as this particular uterus was concerned, there was certainly no proof that it had borne a child, but that, on the contrary, the evidence, doubtful and unreliable as it was, tended rather in the opposite direction, and justified an expression of opinion to that effect, and that, therefore, the accused was entitled to the benefit of any doubt that existed on this point.

These were the thoughts that influenced me in the evidence that I gave, and they were, I believe, warranted by the present state of our knowledge on this subject. On the Continent, and especially, I think, in France, greater weight is attached to the convexity of the internal aspect of the uterine walls as indicative of a nulliparous uterus, than is perhaps usually ascribed to it in this country, but possibly more would be thought of it here in cases where issues so momentous are involved, as in the present case, than under ordinary circumstances. Then, again, the conditions of the uterine walls as to thickness must, I think, be allowed to have some weight. In the present instance they were, as I have said, very thin ; and although no doubt the process known by the term super-involution, which occurs sometimes after delivery, might have brought about this state of things ; yet, I think, preternatural thinness may be taken rather to support the view I took, though the exact opposite was confidently stated by one of the medical witnesses, viz. that thinness of the uterine wall was indicative of pre-existing gestation. On this point, also, I ask the opinion and experience of the Fellows present.

Lastly, there is one question of a medico-ethical character which I would like to be allowed to mention. Personally, I was most unwilling to be mixed up in any way with this now celebrated trial, but having in an unguarded moment, in the early history of the case, about a fortnight after the discovery of the body, consented to examine the remains with special reference to this par-

ticular point, and having written an opinion which, at least, called in question the view taken by the medical witnesses on the other side, and expressed very confidently by one of them, I had at last by the force of a subpœna no choice left me but to appear as a witness for the defence. This, however, involved me in further difficulties, because in common I suppose with most persons who had read anything at all about the case, I had formed some opinion upon it which I am bound to say did not support the evidence which I felt called upon to give after my examination of the remains. Of course, it was not for me, as a scientific witness, to express any opinion upon the merits, or rather the demerits, of the case: nor would it, I conceive, have been right for me to allow my private opinion, formed upon very imperfect evidence and before the defence had been stated, to influence my judgment on a purely scientific question, especially as I knew the defence intended boldly to dispute the identity of the remains with the body of the person supposed to have been murdered. Upon this point also the opinion of the Society would, I think, be sufficiently interesting and important to justify the few remarks I have ventured to bring before it.

Dr. Edis inquired whether any notice had been taken of the condition of the ovaries. He thought some inference might have been drawn from their appearance.

Dr. Palfrey wished the discussion to be postponed, as he was collecting statistics bearing upon the subject. He had taught, and been taught, that the condition of the cervix was some guide as to previous pregnancy or not; but he was rather inclined to believe that the condition of the os was not so clear in deciding the question. It was far too important a matter to be discussed in a few minutes. He therefore proposed an adjournment.

Dr. Aveling suggested that the presence of a large polypus or other morbid growth in the uterus would modify the condition of the cervix.

Dr. Barnes seconded the proposal to adjourn discussions upon the scientific questions as to the evidences of pregnancy; but he wished to express his satisfaction at the course taken by Dr. Meadows in laying a formal statement of his views before his

professional brethren in a form that would be understood and appreciated, instead of trusting to the garbled reports of newspapers and the distortions to which scientific evidence is liable under the manipulation of counsel.

The discussion was then adjourned to a future meeting.

NOTES OF A CASE OF RUPTURED VAGINA DURING LABOUR, WITH RECOVERY.

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Cases of rupture of the vagina are happily so rare, and unhappily so fatal, the mortality being stated to be seven in eight, that any contribution to the literature of the subject is of considerable value, as it is only by a careful consideration of the attendant circumstances, and the method of treatment in favorable cases that we can hope to lessen the mortality.

On Tuesday, March 3rd, 1874, I was sent for by a medical man to assist him in a case of placenta prævia. I arrived in about an hour, and found that as the hæmorrhage had recurred with some severity, he had been already compelled to deliver by turning, and that the labour was, therefore, completed and the placenta removed. I will, however, first give his account of the case up to the time of my arrival.

Mrs. M—, æt. 33, in labour prematurely about the seventh month with her fourth child. She was a tall, pale, thin, and delicate woman, had been married some years, and had been pregnant three times before; had aborted each time at periods varying from four to six months. There was a history of syphilis on the husband's side (prenuptial), and a less clear history of what was probably syphilitic infection of the wife early in their married life.

On each occasion of abortion there was some abnormality or other about the labour over and above the mere fact of the abortion, and always considerable hæmorrhage. On the other occasions she had only with great difficulty recovered. On his arrival there was severe hæmorrhage, and he found on inquiry that there had been more or less for two days previously, and occasionally for a day at a time, for several weeks. On examination he found the os uteri dilated to rather more than the size of half-a-crown, soft and dilatable, the placenta presenting and blood pouring away freely. At this juncture he sent for me. He wished, if possible, to wait for the os to furthur dilate naturally, but the amount of hæmorrhage continued to be so great that this became impossible. He therefore proceeded to dilate the os uteri and then to detach the placenta, which was done without any difficulty; he then turned. The legs, abdomen, and thorax had passed out of the uterus (there had been no pains), but owing to the small size of the premature fœtus, very gentle traction only had been required. At this point he paused for a few moments in the process of delivery, when suddenly a violent, and, as it were, spasmodic pain occurred; the patient with a scream exclaimed that she was "being torn in pieces," and the head was at the same time expelled. After waiting for some little time, during which the patient complained of being in great pain, he removed the placenta without increasing the woman's distress. Very shortly after this I arrived.

On my arrival he told me that the labour was over, but that there was still something irregular to be felt that he could not quite make out. I at once examined and found a large cavity filled with clots, which gave me the impression of a rupture. Without then pressing the examination further, I told him of my fear, and requested him to examine again, when he also recognised the lesion. I then made a more careful examination, and passed my fingers through a rent in the vaginal wall on the left side of the cervix uteri, felt the brim of the pelvis, got also a loop of

intestine between my fingers, and finally felt the upper surface of the fundus uteri, which was well contracted, and on withdrawing the fingers felt the cervix and the contracted os uteri.

The diagnosis was then perfectly clear, and as there was no further hæmorrhage I enjoined the following treatment: —Having regard to the position of the rupture, viz. on the left side of the uterus, as the woman was still lying on her left side, I recommended that she should be left absolutely undisturbed and kept in the same position for at least forty-eight hours, and with this object, that the weight of the uterus might by its pressure towards the left cul de sac keep the rent edges in apposition and so promote union, if possible, by first intention.

This was fairly carried out: the patient remained as she was on her left side for twelve hours; then owing to cramp she shifted her position; but, with the exception of occasional change for relief, she lay for several days on the left side. In addition, opium was freely administered, and when I saw her five days afterwards she was, notwithstanding some inflammatory symptoms, doing fairly well. Peritonitis then supervened, poultices were applied, opium was persevered with, some calomel was administered, and the vagina frequently washed out with Condy and warm water, as there was an offensive discharge. She continued very ill for several days, the symptoms assuming a somewhat typhoid type, the tongue being covered with a dry, light-brown incrustation, making deglutition difficult. There was a tendency to diarrhœa, meteorismus, and marked exacerbation and defervescence, night and morning. Bark was administered, the vaginal injection changed to glycerine of carbolic acid and water, poultices were continued, and at last the symptoms gradually abated. The discharge continued free and purulent; the tongue was raw, swollen, desquamated, and very tender. She continued to improve, and by April 12th all discharge had ceased. By May 14th she had perfectly recovered, and on the 26th went into the country.

From the day of her confinement there had been no vaginal examination lest there should be any undue interference with the parts. On July 7th we again met in consultation, when I examined and felt in the left vaginal cul de sac, about one inch and a half from the cervix uteri, an indurated cicatrix. On passing the speculum I exposed the scar of the rupture: a triangular, puckered, well-marked cicatrix, leaving no doubt, had any already existed, of the severe nature of the accident from which the patient had so happily recovered.

The practical therapeutical lesson I would draw from this case is that in all cases of rupture of the vagina the patient should be placed in such a position as that the uterus should lie above the seat of the rupture, so that by its weignt it might keep the edges of the rupture in apposition, and that this position, however irksome, should be rigorously maintained, and opium freely administered for at least forty-eight hours, so as to promote, if possible, early union.

If this point is carefully attended to in every case I venture to hope that the reputed high mortality in these unfortunate cases may be materially lessened.

A CASE OF SPONTANEOUS RUPTURE OF THE VAGINA, WITH RECOVERY.

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Lacerations of the vagina involving the peritoneum are so rare and the results usually so disastrous, that I am desirous of recording a case in which the accident was

spontaneously produced, and in which, notwithstanding the gravity of the attendant conditions, an excellent recovery was made.

Vaginal lacerations not involving the peritoneum are probably only too common; and it may be that the graver sort occur more frequently than is generally suspected. The accident might easily have escaped detection in the case about to be narrated, and as the patient recovered it would, in that event, probably have never been known that she had been the subject of so grave a lesion. The result of this and other similar cases would encourage the belief that with careful management the accident may not be quite so serious as it is commonly thought to be. The case occurred in the practice of the British Lying-in Hospital, the patient being first attended by the matron and a pupil.

Mrs. C—, æt. 38. Nine children: last fifteen months ago. Present labour commenced at 6 p.m., November 21st, 1874. Waters broke at 9.30 a.m. (next morning), November 22nd. Arm presentation; pains regular and very strong; no progress. Pulse 100; tongue dry.

Was seen by Dr. Wiltshire at 11.45 a.m. on November 22nd, 1874. The right arm, the right leg, and the head were then presenting. The promontory of the sacrum was found to jut out, walnut-like, into the brim of the pelvis.

Posteriorly, and to the right, at the junction of the vagina with the cervix uteri, there was a laceration large enough to admit three fingers. There was very little hæmorrhage.

The part of the fœtal skull which was in contact with the promontory of the sacrum, viz. the left parietal bone, was considerably depressed. The right leg and arm were low down in the vagina. There was no pulsation in the funis, which was also low down. The left foot could just be felt in the upper and posterior part of the uterine cavity, but it could not be grasped; in fact, it could barely be touched.

It will be seen that the case was at once grave and difficult. Considerable embarrassment had been occasioned before my arrival by the disagreeable conduct of the hus-

band, who behaved rudely to the matron and a pupil of the hospital, and to the nurse. His behaviour was offensive and threatening, and he imputed blame to them when they sent for my assistance, declaring that they were the cause of the difficulty. The poor woman herself was in a very anxious and terrified state. She had, it appeared, been delivered by instruments in former confinements, and when she knew that assistance had been sent for in the present labour, she made violent efforts to effect delivery before my arrival. These efforts, it is needless to say, were fruitless to effect the purpose she desired; there can, however, be but little doubt that they were the cause of the laceration. It is fortunate, seeing the character of the presentation, which in shape roughly resembled that of a pyramid with the apex downwards, that the uterus was not forced completely off the vagina. It was clear from the further course of the case that this was an accident which might very readily have happened.

Owing to the offensive conduct of the patient's husband, and the apparent probability of a fatal result, I at once made up my mind that I would neither use instruments of any kind, nor give chloroform. I did so because in the event of death unhappily occurring it might have been said, had I used instruments, that the laceration had been caused by them; and, had I given chloroform, that the patient's senses had been taken away, and that she knew nothing of what was done to her.

Under these circumstances it was clear that the proper course to pursue was to turn the child. For this purpose it was necessary to bring down the left leg, which was still high up in the uterus, as traction on the right leg would only wedge the body more and more in the pelvis.

The heel of the left foot could just be touched by the fingers, and under ordinary circumstances the limb might have been brought down in a second or two. However, under the existing circumstances, it was obvious that the slightest upward pressure of the operator's hand would have strained the utero-vaginal connections, and might have

largely extended the rent. With a blunt hook the limb might have been brought down directly, but I had, for the reasons already given, determined to refrain from the use of instruments. I then made careful and patient attempts, by combined internal and external manipulation, to secure the foot. This was at length accomplished by suddenly but lightly thrusting the limb downwards by the external hand so as to place it within the grasp of the internal hand, the two forefingers of which were kept at rest just within the uterus and without putting the least strain on the vagina. To effect this it was necessary to take advantage of the interval between a pain when the uterus was relaxed. Intervals of relaxation, however, were few and far between, for the uterus was in a very irritable state and was readily excited to contraction. Patience, with a determination not to be baffled, nor to be tempted into the employment of other than the gentlest measures, at last enabled me to succeed. Labour was then speedily completed, the placenta immediately following the fœtus. There was no unusual hæmorrhage.

Careful examination of the rent showed it to be in no way enlarged. Intestine could easily be felt when the fingers were passed into the rent, but no blood clots were found. Some brandy, opium, and a little ergot were then given. No ergot had previously been given by the matron or midwife, nor, I need hardly say, by myself.

The patient's general condition at the conclusion of the labour was very fair. She was low spirited, but not faint. She did not complain of any particular pain, but only of a feeling of soreness within the privates. She had a pretty good pulse. Ordered to lie on back as much as possible.

At 6 p.m., about six hours after delivery, the pulse was 100, and the temperature 98·8° in the mouth. Discharges natural. Has passed water; makes no complaint of pain. General state good. To have opium and quinine.

November 23rd, morning.—Has passed a fair night, considering that her husband and four children slept in the

same bed with her. Pulse 112; temperature 99·6°. Has passed water; discharges quite natural in amount and colour; no offensiveness; tongue moist; abdomen rather tender to touch; not swollen; no vomiting; coughs a little; and says that coughing hurts her just under the navel.

At 4.45 p.m. on the same day the pulse was 100 per minute, and of good volume; tongue clean and moist; no pain, and scarcely any tenderness of the abdomen. The abdomen is, however, rather full. The patient says she has not passed flatus; has passed water freely; skin moist and not hot; discharges natural.

9 p.m., same day.—Pulse 112; temperature 99·6°. Passed water freely; discharges natural; tongue clean and moist; is not feverish; seems very comfortable. To continue opium and quinine.

24th (second day after confinement), 10.45 a.m.—Pulse 100; temperature 99·6°. Tongue clean and moist; lips dry; teeth clean; no headache; no vomiting; has passed water several times during the night and this morning. Abdomen not large, nor tender; discharge offensive, not scanty, but "dirty" coloured. To have a tepid injection of Condy's fluid and water into the uterus. Some soreness of the external parts. To continue medicines.

4.15 same day.—Pulse 100, good volume; temperature 101°. Tongue clean and moist; abdomen soft, not tender, but pain is complained of in right hypogastrium when movement is made; has passed water freely, also flatus; discharges not offensive; complains of cough being very troublesome. To have an opiate linctus; to take a dose of quinine and opium at night. Is perspiring, but not profusely.

9.30 p.m.—Pulse 112; temperature 100·8°. Tongue clean and moist; lips dry; skin rather hot and dry. Complains of a feeling of soreness in the right side of the abdomen; discharges offensive and "dirty" coloured. Injections of Condy's fluid to be repeated. Has passed water freely; no headache; no vomiting, nor has she ever had any; very thirsty; cough easier.

25th, 11 a.m.—Pulse 114; temperature 100·4°. Skin hot and dry; lips dry; severe pain in right side of the abdomen; also tenderness; movement causes much pain; discharges offensive, and of a pinkish watery colour; has passed water and flatus freely. Says she had no sleep last night, and attributes this to the vaginal injection; appetite very good; cough easier.

4.15.—Patient was found drinking tea and apparently going on well; but at 9.30 the same evening she was found to have a temperature of 101·2°; the pulse being 104; the skin was hot and dry; and the head hot, but there was no headache, nor was there any pain anywhere. The abdomen soft; the discharges were watery, but not offensive; cough much better.

26th, 1.15 p.m.—Pulse 112; temperature 101·6°. Complains of heaviness over the eyes; skin hot and dry; lips dry; tongue moist and clean; has passed water freely, also flatus; discharges less offensive, and of a pale greenish colour; abdomen not painful; can move about the bed well now; slept very well last night; appetite good; rather thirsty; breasts beginning to fill with milk.

27th.—Patient going on very well indeed; bowels not open; tongue clean; pulse quiet; skin cool and moist; no pain or tenderness; discharges natural. The patient when visited later in the day was found in tears, owing to a quarrel with her husband, whom she accused of ill treating her. Cough much better; urine and flatus passed freely.

28th.—Pulse 100; skin cool; tongue clean and moist; complains of headache. No pain in the abdomen; discharges colourless and scanty; bowels feeling uncomfortable as if they would be moved. Cough almost well; milk going away nicely without trouble; urine and flatus passed freely. Is in good spirits; appetite good; is not thirsty as she has been other days.

From this time the patient went on very well and made a good recovery.

The matron of the hospital reports to me that Mrs. C—

has again been delivered prematurely (at the eighth month) of a child without difficulty.

Remarks.—The chief points of interest in this case appear to be—

1. The spontaneous occurrence of the rupture.
2. Its cause.
3. Its seat.
4. The singular freedom from unfavorable symptoms subsequently, and the excellent recovery the patient ultimately made.

1. With reference to the spontaneous occurrence of the rupture, it may be said that in all probability the majority of the *circular* lacerations which take place at the junction of the cervix uteri with the vagina during labour are of similar origin, that is, that they originate spontaneously. They may, of course, be produced by operative procedures, but it appears from the records of cases that they generally arise spontaneously.

Dr. West * mentions a case of fatal rupture of the vagina at the end of pregnancy, but before labour had commenced, which was related by Dr. Doherty in the 'Dublin Hospital Gazette,' May 15th, 1845. The patient had a contracted pelvis, and her vagina was in an unhealthy state as the result of the severity of previous labours. Post mortem the os uteri was found closed, and the rent in the diseased vagina corresponded to the right linea-ilio-pectinea, which, however, was not sharper or more prominent than natural.

Dr. West also mentions a case of laceration of nearly half of the vagina from the cervix uteri, in which the rent healed speedily, and the patient left her bed at the end of four weeks. This case occurred to Dr. Majer.

In close connection with the spontaneous occurrence of the laceration must be considered—

2. *Its cause.*—In this case three things conspired to produce the laceration, first, the projecting sacral pro-

* "Report on the Progress of Practical Medicine in the Departments of Midwifery and the Diseases of Women and Children during the years 1844-5," 'Brit. and Foreign Med.-Chir. Rev.,' vol. xx.

montory; secondly, the mal-presentation of the fœtus; and, thirdly, the violent efforts which the patient made to effect delivery before assistance could be rendered her.

The pelvic deformity was doubtless the cause of difficulty for which instrumental help became necessary in former labours. It was of that slight character which seems to be so much more efficient in the production of such lacerations than are the graver forms of pelvic deformity. A slight, though marked diminution in the capacity of the pelvis is a common factor in the production of these lacerations.

Then as regards the mal-presentation, while beyond doubt it contributed largely to the production of the accident in this particular case owing to the pyramidal form the fœtal parts assumed, yet it certainly is an unusual presentation.

Dr. McClintock, in his admirable memoir on "Lacerations of the Vagina" ('Dublin Quarterly Journal of Medical Science,' May, 1866), says that in every instance that he knew of where vaginal laceration was spontaneously produced the head was the presenting part. In the case now under consideration the head, hand, and foot were simultaneously engaged in the pelvis.

The violent expulsive efforts made by the patient must have aided largely in the immediate production of the laceration.

In this connection it is interesting to note that Ingleby (p. 182) mentions rupture of the diaphragm as having occurred during labour; doubtless from effort.

As regards the causation of vagino-uterine lacerations generally, all observers are agreed that several factors may be concerned in their production. Thus, Ingleby ('Obstetric Medicine,' p. 177) justly lays stress on "want of correspondence between the axis of the uterus and axis of the pelvis" as a cause of laceration both of the uterus and of the vagina, and especially of the latter. This, as he remarks, was "pointedly stated by Douglas" as a cause of the accident.

Dr. Braxton Hicks has very ably pointed out the manner

in which the accident may be occasioned in an excellent paper on the subject in the 'Lancet,' January 23, 1869.

3. The seat of the rupture was unusual. Generally the left side of the vagina is torn. Dr. Matthews Duncan ('Contributions to the Mechan. of Natural and Morbid Part.,' p. 267, 1875), in discussing the changes undergone by the cervix uteri during labour, says that the body of the uterus in its regular contractions acts on the cervix and on the vagina equably and at every part, pulling upon them and pushing the fœtus into and through them, and that in accordance with this condition of matters the vagina, when lacerated by uterine efforts, as it is not very unfrequently, is torn transversely to the direction of its axis, while the direction of the force propelling the fœtus through a curved passage leads to the posterior part of the vagina being the ordinary seat of the laceration, and also to the tear generally implicating the left more than the right half of the vagina. And when speaking of the curves of the developed genital passage (p. 51), he says that the angle of deviation of the axis of the uterus from the axis of the brim of the pelvis has been "looked upon as affording some explanation of the alleged comparative frequency of laceration of the cervix on the left side in ordinary labour." He further says: "It may explain the greater frequency of laceration of the vagina on the left than on the right side, as pointed out by McClintock." And, again, Dr. Duncan says (p. 54), that the last curve of the developed genital passage, that extending from about the middle of the pelvis to the outlet from the soft parts, "leads to the explanation of the greatest frequency of lacerations in the posterior wall of the uterus and vagina."

It may here be added that Dr. Duncan states (p. 95), that the *uterus* is never spontaneously ruptured when it is healthy, an opinion in which I have hitherto been somewhat disposed to concur, though the opinion recently expressed by Bandl,* Barnes, and others, would seem to

* Bandl ('Centralblatt,' No. 33, 1875) gives his views on thirteen cases of rupture of the uterus observed by himself, and nineteen obtained from the

point to an opposite conclusion. The same cannot be said of the vagina, however, where mechanical injury appears to play the most important part.

Dr. McClintock, in the able essay already alluded to, justly remarks, that "it is worthy of notice that in nearly all the cases where the laceration of the vagina was of spontaneous origin it had taken more of a circular than longitudinal direction. In uterine lacerations, on the contrary, the prevailing direction is more or less longitudinal."

Leishman says (p. 490) of rupture of the uterus, that considerably more than half of all the ruptures at the full time occur in the region of the cervix, generally of that part which marks the junction between the uterus and the vagina."

Dr. Braxton Hicks (loc. cit.) remarks, "If, then, any part of the vagina cannot bear the strain of the uterus driving itself backwards it gives way, and, as we should *à priori* expect, in the transverse direction." Dr. Hicks also says that obliquity of the womb favours laceration of the side to which the head inclines.

It may here be permissible to remark that in all probability attrition of the utero-vaginal tissues between the sacral promontory and the fœtal head played some part in the production of the rent in the case we are now considering. The rupture occurred at the junction of the vagina with the cervix uteri, just where the tissues are thinnest, and it involved the whole of the structures at this spot. There was, therefore, laceration of the peritoneum, and through this rent coils of intestine were felt. It is singularly fortu-

records of the Vienna Lying-in Hospital. He says that in no case was there evidence of pathological change in the substance of the uterus. He admits, however, that the cervix was unusually thin, and that the fissure was mostly found in the cervix, or began there when the body of the uterus was also torn. He doubts, absolutely, predisposition from disease of the uterine wall, believing the final cause to be disproportion. He regards pressure as producing sloughing, and not rupture.

Chiari, Braun, and Spaeth are said to agree with Bandl in regarding excessive thinning of the cervix as the chief abnormality.

nate that there was no prolapse of the bowel, which would, undoubtedly, have materially augmented the gravity of the case. Dr. Leishman, in his admirable work ('System of Midwifery,' 2nd edit., p. 490), says "that recovery has taken place even when a considerable coil of intestine has passed through the wound and occupied the vagina;" and other instances of a like fortunate result are on record. Still, prolapse of the bowel is a very grave complication, and appears to be the cause of a fatal termination in many cases. Fehling ('Archiv für Gynäk.,' B. vi, s. 103) mentions a case in which a patient, attempting to replace a prolapsed vagina, ruptured the posterior cul-de-sac, and died of protrusion of intestine which could not be reduced.

Upon the implication of the peritoneum, Dr. McClintock makes the following valuable remarks:—"The cases in which it escapes are very rare, and where the tear engages the posterior region of the peri-uterine vagina it is hard to conceive it possible for the serous cavity to avoid being opened." He goes on to say, however, that the facts he mentions would seem to show that "whether the peritoneum be engaged or not has comparatively little influence upon the result," a conclusion with which it seems difficult to agree, since it is clear that prolapse of the intestine, which unquestionably adds greatly to the gravity of the lesion, cannot occur when the peritoneum remains entire. Indeed, Dr. McClintock's remarks on treatment, to be referred to presently, would appear to show that he considers that closure of the rent by art would greatly influence the result; and it is obvious that operative interference is much more urgently demanded in the one case than in the other.

The suddenness with which the rent occurs appears to affect considerably the implication or escape of the peritoneum, which, as Dr. Matthews Duncan says (p. 98), is capable of growing or distending slowly to any extent, but appears to be incapable of sustaining rapid distension. A case mentioned by Dr. Radford, in his admirable memoir on "Vaginal and Uterine Ruptures," in our 'Transac-

tions,' shows that the peritoneal membrane may, however, sustain considerable and rapid distension without giving way, for in one of his cases of rupture of the uterus the fœtus was found lying in a sac of peritoneum, and completely extruded from the uterine cavity.

4. The singular freedom from unfavorable symptoms subsequently, and the excellent recovery the patient ultimately made, are points of interest.

In spite of unusually unfavorable environments and the presence of a troublesome cough, which especially caused us anxiety, because of the fear that it might occasion extrusion of bowel, the patient progressed almost as satisfactorily as she might have been expected to do after an ordinary labour.

Dr. Gaillard Thomas has recently published a case of recovery from what he calls "peritoneal rupture," *i. e.* rupture of the vagina at its junction with the uterus ('Amer. Journ. of Obstetrics,' August, 1875, p. 325).

The patient was deformed. Unsuccessful attempts had been made to deliver with the forceps. Dr. Thomas perforated and used the cephalotribe. The intestines had descended through the rent. Dr. Thomas mentions that he had only seen two cases of recovery from vaginal rent, and in one the peritoneal cavity was not opened.

Rupture of the vagina sometimes occurs in the unimpregnated state. Thus Schroeder, in Von Ziemssen's 'New Cyclopædia of Medicine,' vol. x, p. 512, states that Gotthardt ('Wiener Med. Wochenschr.,' 1869, No. 94) met with a case of spontaneous rupture from a fall; and (p. 215) that the cervix uteri may be ruptured in the unimpregnated state by the amount of strain brought to bear upon it in an upward direction by the growth of ovarian, fibroid, or other tumours. He further mentions (pp. 220-1) that a rent of the vagina occurred in an attempt at replacement of an inverted uterus in Dr. Gaillard Thomas's practice. The patient recovered.

There seems to be a general consensus of opinion amongst authors of note that these lacerations are more

common than is generally believed. Whether or not this is so, and whether the accident is as fatal as it is generally thought to be, are points calling for further investigation. Meanwhile particulars of cases will be useful as forming a basis for sound conclusions.

Two things strike one as being common and important factors in the production of these lacerations, viz. first, a slight, though marked, diminution in the capacity of the pelvis, and, secondly, the administration of ergot.

Attrition of uterine or vaginal tissue against a projecting bony surface is no doubt an efficient cause of laceration. It is usually thought that the danger from this source is greatest in long and severe labours; and while there are doubtless good grounds for this belief, yet it has appeared to me that laceration may be determined by the bony projection early in a violent labour, when either the uterine contractions are of themselves very strong or unduly forced by ergot, or, as in my own case, the voluntary efforts are excessive.

In reference to the very important question of treatment we have for our proper guidance to take into consideration several points, and here we must be guided chiefly by principles, since the combined practice of all observers is insufficient to form the ground-work of a plan which shall on all hands be admitted as reliable. There are points on which all observers are agreed, and they at once commend themselves to our judgment. Such, for instance, is the recommendation that delivery should be accomplished as speedily as possible, though there may be some difference of opinion as to the best method of effecting this.

As regards the treatment of the rent itself opinions are conflicting. It would, at first sight, appear that the rent should be at once closed by sutures; and, theoretically, this is correct. Two difficulties, however, have to be encountered, the first and greatest being the safe introduction of the sutures. There need be no great difficulty in introducing the sutures themselves, but unquestionably there is considerable difficulty in avoiding the wounding, or inclusion, of

intestine. No one who has not had experience of a case can well imagine the difficulty which would attend the passage of an unprotected needle through the edges of, and across, the rent without pricking the gut. I think the sutures might be safely introduced, however, by means of an aiguille caché, such as I hoped to have been able to present.* The sharp point should only be exposed during the perforation of the edges of the rent, and it should then be guarded by the finger of the operator. In this way the chasm might be safely traversed without the least risk of wounding the gut even though some coils were encountered. Of course, extreme care should be taken to avoid inclusion of bowel in the rent while tightening the sutures.

The other difficulty which may attend closure of the rent is the risk of incarcerating or imprisoning blood within the peritoneal cavity. I do not think this need be regarded as a very formidable obstacle, for the probabilities are that, vascular as the tissues undoubtedly are, bringing the edges of the rent together by sutures would, to a large extent, control whatever hæmorrhage there might be.

Fortunately, the tendency to the escape of blood externally is greater in this than in uterine lacerations; and it is hardly necessary to urge the propriety of careful and gentle removal of clots before finally tightening the sutures.

Dr. McClintock justly aims at remedying the accident as far as possible by art. He says, "It can scarcely be doubted that the permanent retaining of the torn parts in apposition would conduce to the chances of the patient's recovery; and, with proper instruments, I really think there could not be any great difficulty (the vagina being so relaxed and capacious) in making two or three, not more, stitches with wire suture just to hold the edges of the laceration together." I think I should myself prefer to use silk, such as Mr. Spencer Wells employs in his wonderfully successful ovarian operations. This should be quite pure, and is much more easily manipulated than wire sutures.

Dr. McClintock refers to two cases of Dr. Marion Sims,

* The instrument has not yet been completed.

which have a direct bearing on this question. In each the peritoneal cavity was accidentally opened during operations on the uterus; and in each case the opening was closed by metallic sutures; both patients recovering. Dr. Marion Sims removed blood which had forced its way into the peritoneum by means of sponge probangs. Should sponges be used for this purpose, exceeding care should be taken to have them quite clean and wholesome.

Dr. McClintock, in ably advocating the insertion of sutures, says, "Practically, the only difficulty I see in the way of carrying into effect the recommendation, supposing we have the necessary instruments at hand, is how to command a sufficiency of light, as, of course, the patient cannot be moved; but this difficulty is not of an insuperable kind." Were light absolutely essential I do not think there need be any great difficulty in supplying it. The experience afforded by my case would lead me to doubt the absolute necessity for light.

Judging by that case, I am disposed to think that with the aiguille caché sutures might quickly and safely have been introduced by touch alone. This plan, should it prove practicable, would have the advantage of avoiding unnecessary movement of the patient. There would also be less risk of the introduction of air into the peritoneal cavity. Here the propriety of attempting drainage of the peritoneal cavity after laceration of the vagina obtrudes itself. Ought we, in closing the rent, to intentionally leave a channel whereby intra-peritoneal fluids may escape, and if so, what is the best method of accomplishing this? Upon this point I should be glad to learn the opinions of Fellows of the Society. The success which has attended the plan of peritoneal drainage in ovariotomy is, perhaps, hardly sufficient to warrant a recommendation of its systematic adoption in lacerations of the vagina involving the peritoneal cavity. Should the plan appear desirable in a given case, I would suggest that it might be efficiently carried out by means of a piece of india-rubber drainage tube, through which, if necessary, disinfecting fluid might

also be injected. I do not, however, consider that we should be warranted in deliberately leaving an opening. At any rate, more light is required on this point. Of course if there be evidence of poisoning from retention of blood, &c., within the peritoneal cavity, steps should immediately be taken to evacuate the pent-up materials.

Opium is clearly indicated, though not, perhaps, in the very large doses which are apt to be employed under such and similar circumstances.

I must ask the indulgence of the Fellows of the Society for having occupied their attention so long, and would express the hope that they will kindly give the meeting the advantage of their knowledge and experience on the subject of vaginal lacerations.

Dr. Barnes wished to state his objection to accept in its absolute sense the doctrine that the uterus never ruptured unless its tissue was diseased. He himself had seen cases where no disease could be detected. He might distrust his own competency as an observer; but he had submitted specimens of one case to examination by Drs. Bristowe and Montgomery, who confirmed his observation. The conditions under which the uterus sometimes burst were quite compatible with the hypothesis of sound structure. Sudden violent contraction of the uterus upon fluid contents would, as in the case of the Florentine experiment of hydrostatic pressure inside a golden globe, cause rupture, unless the fluid escaped by a natural opening.

Dr. Palfrey had met with cases. He objected to the use of the syringe in these cases. In one instance, immediate collapse had occurred in consequence. Nothing would induce him to sanction it again. The irrigator answered every purpose where it was necessary to wash out the vagina. If the rupture favoured prolapse of the intestine, he advocated sutures.

Dr. Edis referred to a case where turning and attempt at extraction had been resorted to in a primipara, the antero-posterior diameter of the pelvis being less than two inches. After prolonged efforts, the body was extracted, the head being left behind. When Dr. Edis examined the patient, the hand passed readily through a large rupture in the upper part of the vagina and neck of the uterus into the peritoneal cavity, the head of the fœtus lying close under the liver. The patient died. He mentioned the case as illustrating the necessity of carefully determining, on first examination, the exact condition of the pelvis.

Dr. Barnes inquired if, in Dr. Wiltshire's case, the child was dead before rupture took place, because death of the child was often a cause of rupture.

Dr. Wiltshire replied, that the funis had ceased to pulsate before the rupture. The patient had been kept on her back to prevent the descent of intestine.

CASE OF CÆSARIAN SECTION FOR DEFORMED PELVIS.

By Dr. James W. J. Oswald.

Eliza P—, æt. 29, a deformed rickety subject, was kept by her brother till she married, in October, 1874, a railway servant, who is one of the few survivors of the celebrated Six Hundred who charged at the Battle of Balaklava. In due course she became pregnant, and in October, 1875, she sent for me as she was "swelling very much" as she called it.

On my visit I found her suffering from general œdema, and in expectation of being very shortly taken in labour. I noticed her peculiar condition at the same time, but did not then make an examination to ascertain whether all was right in the pelvic region. I prescribed some simple diuretics, and recommended her to lie down as much as possible.

On October 23rd, at 11 a.m., I was summoned to her by her husband, who informed me that she had been in great pain nearly all night. On my arrival I found that labour was really going on, and that the uterine contractions were very severe. On examination per vaginam I found a hard osseous mass projecting from the sacrum, and filling up a great part of the pelvic cavity, leaving little over an inch in space between this bony mass and the pubes. I found the os uteri high up, slightly dilated, and the membranes protruding. Finding that delivery "per vias naturales" was impossible, and seeing clearly that the Cæsarian section was the only means of effecting delivery, and giving my patient the chance of her life being saved

as well as that of the child, I sought the assistance of Dr. Routh and Dr. Rogers, who kindly came and saw my patient. After a minute examination made per vaginam as to the state of this enlargement, and as to the probability of being able to deliver by the cephalotribe, a consultation was held, when it was decided that the only way to effect delivery was by performing the Cæsarian operation. At 4 p.m. the patient was placed under chloroform by Mr. J. Sydney Pearse, of University College, lately one of my assistants, and the operation was commenced by Dr. Routh, who was assisted by Dr. Rogers, Dr. G. Wilds Linn, of Philadelphia, and myself. Messrs. R. J. W. Oswald and Garrett, of Charing Cross Hospital, being present.

The catheter was with some difficulty passed, but no urine was found to be in the bladder. An incision was made from two inches above the umbilicus to within three inches of the pubes. The parts were most carefully divided until the peritoneum was reached; this was divided on a director. The distended uterus was soon brought into view, and appeared whiter than normal. It was impossible to detect the placental murmur, so that an incision was made through the walls without any loss of time. The placenta was the first to present itself, but was soon removed, and on trying to lift the child it was found that the head was so firmly grasped, partly by the neck of the uterus, but also in some manner by being wedged in the antero-posterior diameter, that it could not be extracted by traction of the body of the child. Dr. Routh then applied Smellie's forceps round the child's head. Not the slightest difficulty occurred in so doing, the blades being placed on each side at each end of the transverse diameter; traction was now made and the head delivered. The uterus contracted at once, except quite at the lower portion, where some bleeding took place, and part of the uterine fibres had probably been torn. Some difficulty occurred at this stage of the operation by the intestines being pushed before the uterus. These were, however, carefully replaced, and the

parts were quickly sponged and closed. The uterine walls being brought together by carbolized gut sutures, and the abdominal by silver wire, and dressed with cotton wadding and strapping, a moderately tight bandage being applied over all. Very little blood was lost during the whole operation. She quickly recovered from the effects of the chloroform, and expressed herself as being tolerably comfortable, and not in very much pain.

7 p.m.—Pulse 130, small, compressible; resp. 35, shallow and jerky; temp. 100°. Has complained of cold extremities, but has recovered to a certain extent. Pain in back; spasmodic; none in wound. No hæmorrhage or vaginal discharge. Ordered brandy and egg.

11 p.m.—Pulse 140, very feeble; complains of cold; resp. 34. No sickness; great tenderness over abdomen. Ordered one grain of opium every four hours, and two teaspoonfuls of brandy with tablespoonful of water every hour. Used catheter, but found no urine.

24th, 8.30 a.m.—Patient has passed a tolerably good night; has dozed occasionally, but has not slept soundly; every few minutes is seized with pain seemingly of a spasmodic character. Pulse 140, feeble; resp. 38; temp. 99·8°. Has passed urine during the night. To continue the brandy; to have plenty of milk and beef tea.

12 (noon).—Pulse 128, very compressible; resp. 34; temp. 98·8°. No hæmorrhage; slight vaginal discharge of a mucous nature; countenance pale; tenderness over abdomen, but no acute pain. Brandy and port wine ordered every two hours. Milk and beef tea continued.

9.30 p.m.—Pulse 120, very feeble; resp. 30; temp. 98.2°. Countenance very pallid, slightly tympanitic; vaginal discharge continues, more of a sanguineo-mucous nature. Ordered one grain of opium every four hours; to continue brandy and wine, with the milk and beef tea.

25th, 8.30 a.m.—Patient has passed a restless night. Pulse 120; resp. 29. Has not complained of much pain; urine passed freely.

11.30 a.m.—Pulse 130; resp. 32; temp. 99°. Com-

plains of great abdominal pain and severe vomiting; lochiæ passing freely, not offensive. Ordered an effervescing mixture; arrowroot and brandy to be given in small quantities frequently.

4.30 p.m.—Pulse 130; resp. 32. Vomiting slightly checked by the medicine. The arrowroot and brandy have been retained.

10.45 p.m.—Pulse 132; resp. 34; temp. 98.8°. Vomiting entirely stopped; less pain over abdomen; vaginal discharge free. Continue mixture with arrowroot and brandy, and to have a little beef tea.

26th, 5 a.m.—Was summoned by the husband as he thought his wife was dying, which unfortunately proved to be too true. I found her nearly pulseless, quite unconscious, and I remained with her until 6.30, when she died. On my expressing a wish to make a post-mortem, in order to see the extent of the deformity in the pelvis, the husband at once acceded to my request. It was made the same evening, twelve hours after death.

Post mortem, 7 p.m.—On exposing the external wound it was found to present a perfectly healthy appearance. The wire sutures being removed the uterus was brought into view, but from some unaccountable cause each of the carbolized gut sutures was lying perfectly loose, the knots having come untied. There was about half a pint of serosanguineous fluid in the peritoneal cavity. The body was well nourished, considerable œdema of lower extremities, especially of inner surface of thighs and vulva; no cadaveric rigidity. On opening thorax, heart was raised to the left; pericardium normal, adhesion to left pleura; no increase of fluid.

Heart.—Right ventricle pale, soft, friable, containing clot extending into pulmonary artery for about half an inch; valves healthy. Left ventricle, same structure contained no clot. Right auricle, clot continues with right ventricle.

Lungs, right.—Congested; collapsed at spots at base;

on section full of serosity; no adhesions. *Left.*—Pleura strongly adherent to thoracic walls and to posterior surface of pericardium. Lung much contracted to about one third of normal size; at apex masses of consolidation were found; lung floated in water.

Peritoneum.—No traces whatever of any inflammatory mischief.

Liver.—Normal size, pale, extremely friable; capsule non-adherent; gall-bladder full.

Spleen.—Normal.

Kidneys.—Capsule easily separated; cortical part thickened, pale, opaque; the consistency was diminished; pyramids injected.

Intestines.—Quite healthy.

Uterus with bladder removed and now shown. It presents no trace of any disease, shows well the line of incision made, and presents likewise a small laceration at lower part, evidently made when the head was removed, as it required considerable force to lift it out. After removing all the abdominal viscera the true state of affairs could well be seen. There was considerable angular curvature of the spine, commencing from the second dorsal vertebra. The pelvis was much flattened, and the promontory of the sacrum projecting to such an extent that the widest space between it and any part of the brim measured exactly an inch and three quarters, and the smallest three quarters of an inch. The length of each femur was six inches. There was no curvature of any of the extremities, and the exact height of the patient was four feet.

Dr. H. Smith inquired how far through the walls the sutures were put, what quantity of opium was given, and what was the cause of death. There seemed to have been an absence of peritonitis, and the temperature was never very high. Did death result from septicæmia from the fluid effused into the peritoneal cavity? It was a question whether the abdominal wound should not have been reopened, and the fluid sponged out. He had seen this done in one case successfully.

The President remarked that the value of the paper would have been considerably increased if the measurements of the

pelvis, the curvature of the spine, and the length of the extremities, had been given more in detail.

Dr. Palfrey inquired whether sutures had been placed in the uterine walls.

Dr. Routh could not but believe that the misfortune in Dr. Oswald's case was that he (Dr. Routh) had used catgut ligatures. So far the lesson should be widely published. For although these were of the best kind, and had been kept in carbolized oil, as supplied by Messrs. Krohn and Sesemann, and securely tied, they had loosened, and finally untied themselves, and so a quantity of fluid had passed into the abdominal cavity and poisoned the patient. She died, in fact, of septicæmia and clot in the heart. The rent inferiorly was securely fastened at the time. But he quite felt now that had he used silk or metallic sutures to the uterus instead of catgut, he might have saved the patient.

Dr. Hayes called attention to three loose catgut ligatures being inside the specimen then going round.

Dr. Meadows remarked that this was certainly the second case in which death had resulted entirely from the use of catgut sutures in the uterus. He had himself lost one case which at first seemed to be doing perfectly well, when suddenly the patient went into collapse and died, and on post-mortem examination it was found that all the catgut uterine stitches had given way, the wound in the uterus was found gaping open, and the patient had in fact died an accidental death, from what might be called rupture of the uterus. In future, he would never again use catgut, but wire, for the uterine stitches.

Dr. Cleveland remarked that a common form of weather indicator was constructed on the principle that catgut possesses the property of relaxing in a moist and contracting in a dry atmosphere. He wished to inquire if ligature of this material could not be properly prepared beforehand.

Dr. Rogers questioned whether sutures at all were requisite in the uterus. In two cases he had seen where none were employed, one recovered, and one died. One distinguished obstetrician never uses sutures, and others use silver sutures with success.

Dr. Oswald, in reply to Dr. Heywood Smith, said that opium in one grain doses had been given regularly every four hours from the day of the operation. Milk in large quantities was taken. Beef tea was taken and retained on the stomach from the second day, and brandy and wine in small quantities were constantly administered. The abdominal sutures were not made particularly deep. In reply to the President, Dr. Oswald said that there was no curvature of the extremities; that the exact height of the patient was four feet, and that he would make a future report of the case for the 'Transactions.' In reply to Dr. Hayes, Dr. Oswald said that the carbolized gut sutures had been

removed from their original position, and kept simply to show their condition. In reply to the President, Dr. Oswald said that the pelvis was quite flattened; the largest space, of an inch and three quarters, being on the right side; the smallest, three quarters of an inch, on the left. It was first thought advisable to use the cephalotribe, but on further reflection it was considered utterly impossible to bring the body of the child even in a dismembered state through so small an opening, without producing the most serious results. Dr. Oswald considered death due to embolism, the pulmonary artery being plugged by a large clot.

Report on Organs taken from the Subject of Extra-uterine Pregnancy. Exhibited at the June Meeting by Dr. Galabin.

The organs consisted of the uterus, Fallopian tubes, ovaries, broad ligaments, bladder, and portion of the vagina, together with a large cyst and a fœtus.

The fœtus was full grown. It measured 16 inches in length; $8\frac{3}{4}$ inches from the umbilicus to the top of the head. It had a sodden appearance; the hair on the head had in great part fallen off; the patch that still remained was long and thick; the nails of the fingers and toes were well developed.

The outer surface of the cyst was rough from adhesions; the wall was pale, tough, firm, and composed of fibrous tissue. The cyst was adherent to the posterior surface of the bladder, anterior surface of the left broad ligament, left border, and part of anterior surface and of fundus of the uterus. Between the bladder, the neck of the uterus, and the cyst, was a small partially enclosed space; the outer surface of the cyst at this part did not appear to be covered by peritoneum.

The placenta was attached immediately to the cyst wall at the posterior part. The interior of the cyst was nowhere lined by membrane. The placenta was pale, remarkably

soft, and showed a trabecular structure. It was partially detached by a clot of blood effused between it and the cyst wall.

The uterus, which was retroflexed, had been opened by a longitudinal incision through its anterior wall. It measured $3\frac{3}{4}$ inches in length; anterior wall $\frac{1}{2}$ inch in thickness. The organ was lined by a thin layer of soft tissue, which presented an irregular, finely flocculent surface. In the anterior wall, about $\frac{3}{4}$ inch above the internal orifice, was an opening which easily admitted the finger, and communicated with the cyst which had contained the fœtus. This opening was lined by the cyst wall, which appeared to dip through the anterior wall of the uterus. The wall of the uterus around the opening was firmly attached to the portion of the cyst wall prolonged into it. The membrane lining the uterus terminated abruptly at the edge of the opening, and was not continued into the interior of the cyst.

The right Fallopian tube was natural. The right ovary contained a corpus luteum $\frac{1}{2}$ inch in length, $\frac{1}{4}$ inch in thickness, having white, hard, cartilaginous, folded walls, enclosing a small cavity containing a little dirty fluid.

The left Fallopian tube was free at its outer part, but the inner third was firmly adherent to the cyst wall, but formed no part of it; its canal was perfect, and did not communicate with the cavity of the cyst as was shown by the passage of a bristle along this part of it.

In the centre of the left ovary was a small clot.

From these facts we have been led to infer that the case was one of abdominal pregnancy, and the communication between the cyst and the cavity of the uterus was the result of pressure on the most prominent part of an acutely retroflexed organ; that is, on the prominence formed opposite the angle of flexion.

W. S. Playfair.
John Williams.
Alfred L. Galabin.

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Report of the Board of Regents for 1873 8vo. Washington, 1874 Institution.

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Nos. 1, 2, 3, 1869-70.
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Archives of Science and Transactions Dr. J. M.
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Mémoires et Comptes-Rendus
Tome XIII, 1873, 8vo. Lyon, 1874 Society.

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London, 1875 — Purchased.

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Vol. 1, Nos. 1—2, 8vo. Burlington, Vermont, 1874 — Dr. J. M. Currier.

Annales de Gynécologie (Maladies des Femmes, Accouchements), publiées sous la direction de Pajot, Courty, et T. Gallard, par A. Leblond
Tome 1—2, 8vo. Paris, 1874 — Editor.

Archives de Tocologie, des Maladies des Femmes et des Enfants nouveau-nés; publiées par J. A. H. Depaul 8vo. Paris, 1875

Zeitschrift für Geburtshülfe und Frauenkrankheiten, unter Mitwirkung der Gesellschaft für Gynækologie in Berlin; herausgegeben von Eduard Martin und Heinrich Fasbender
Band 1, Heft 1, 8vo. Stuttgart, 1875 — Ditto.

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HOSPITALS—

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LYING-IN HOSPITAL (Rotunda). Sixth Clinical Report for 1874, by George Johnston
8vo. Dublin, 1875 — Dr. Johnston.

——— Boston Lying-in Hospital, Report for 1875 and Bye-Laws 12mo. 1875 — Dr. W. L. Richardson.

——— Istituto Ostetrico annesso all' Ospizio di Maternita di Parma. Breve relazione clinica e statistica per 1872-4, pel Dott. G. Calderini 8vo. 1873-4 — Author.

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Macclesfield, Annual Reports, by George Bland
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DECEMBER, 1875.

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ANATOMICAL AND PATHOLOGICAL SPECIMENS.

1. Anatomical.

A preparation showing the condition of the uterus and its appendages a few days after delivery, at about the seventh month.

A placenta, with corresponding portion of uterus, showing the utero-placental deciduæ. A description will be found in the 'Lancet,' February 23rd, 1856, p. 204.

Two fœtuses of about four months; one attached by its cord to the placenta, the other enclosed in its membranes.

Fœtus of about three months enclosed in its membranes, with placenta.

Two Surinam toads—male and female: the female showing cellules on her back in which the young tadpoles have been changed into toads.

2. Pathological.

Fibrous tumour of the uterus (see the 'Obstetrical Transactions,' vol. viii, p. 105).

Intrauterine fibrous tumour, removed by the single-wire écraseur.

Fibrous tumour of the uterus expelled by natural processes after abortion.

Segment of a large fibrous tumour (ovarian or uterine), the centre of the growth being occupied by a large cyst, and the containing wall averaging about an inch in thickness (see the 'Obstetrical Transactions,' vol. xv, p. 58).

Specimen of an extrauterine gestation.

An extrauterine gestation, with hypertrophied fimbriated extremity of the Fallopian tube (see the 'Obstetrical Transactions,' vol. xiii, p. 268).

An entire ovum of between two and three months' gestation, which died at that period, and was afterwards retained in utero for three months, when it was expelled.

Fœtus and placenta of about five months, with the funis tied in a remarkable knot round the neck of the child (see the 'Obstetrical Transactions,' vol. xiii, p. 1).

A deformed fœtus, with the abdominal viscera protruding from the umbilicus (see the 'Obstetrical Transactions,' vol. xiii, p. 263).

Preparation showing the parts involved in a case of uterine hæmatocele (see the 'Obstetrical Transactions,' vol. iii, p. 79).

Cysts of the Fallopian tubes (see the 'Obstetrical Transactions,' vol. viii, p. 139).

Specimen of hydatidiform degeneration of the chorion.

An anencephalous fœtus, female.

A "corkscrew" funis.

Two dried ovarian cysts.

A dried blood cyst.

Singular malformation, showing the influence of maternal impressions on the fœtus in utero in the lower animals—in the cow.

Ditto, ditto—in the mare.

3. Pelves, &c.

THE ARTHUR FARRE COLLECTION.

A collection consisting of eighteen pelves and seven casts of pelves, normal and abnormal; four fœtal skulls, and a fœtal skeleton.

Two female pelves of natives of Little Russia.

Pelvis of a female on whom the Cæsarian operation was performed (see the 'Lancet,' October 12th, 1872, p. 523).

A Mohammedan female pelvis.

A Hindu female pelvis and fœtal skull.

Two casts of the pelvis of a Brahmin female dwarf.

Cast of a transversely contracted or 'Robert' pelvis.

Cast of a pelvis.

A collection of sixty-two stereoscopic photographs of interesting pelves and a few fœtal heads, taken from specimens in the Royal University Maternity of Berlin (with an account of their measurements, and a history of nearly all the cases, in MS.).

CASTS.

Four fœtal heads, delivered by the operation of cephalotripsy by G. H. Kidd, M.D. (see the 'Obstetrical Transactions,' vol. xii, p. 1).

Two fœtal heads, delivered by Heywood Smith, M.D. (see the 'Obstetrical Transactions,' vol. xii, p. 134; vol. xiii, p. 37).

Three fœtal heads, delivered by J. Braxton Hicks, M.D. (see the 'Obstetrical Transactions,' vol. xv, p. 41; vol. xvii, p. 49).

Fœtal head, delivered by the forceps in face presentation in the mento-lateral position (see the 'Obstetrical Transactions,' vol. xv, p. 39).

Heads of two anencephalic monsters.

Head of a cyclopean monster (see the 'Obstetrical Transactions,' vol xv, p. 35).

A deformed fœtus (see the 'Obstetrical Transactions,' vol. xiv, p. 139).

Antique group, representing the circumstantials of labour in very early times, taken from the original, found at Golgas, in Cyprus. The group consists of four figures: the mother reclining on the couch, an attendant at her head, and the nurse sitting at the foot of the bed with the new-born infant in her lap. The date of the original sculpture, probably 300 years before the Christian era.

MODELS.

Two cases of wax models, representing (I) the normal and various pathological conditions of the os and cervix uteri, &c.; (II) polypi of the uterus, with their different attachments.

Case I.

OS AND CERVIX UTERI.

1. Normal condition of the os and cervix.

2. Cervix inflamed, without ulceration.

3. Cervix, more intense inflammation; marks of scarification.

4. Slight inflammation within the cavity of the cervix, glands enlarged.

5. Inflammation of the cavity of the cervix.

6. Inflamed glands scattered over the surface.

7. Marks of leech-bites.

8. Slight erosion or ulceration about the os.

9. Ulceration of the anterior lip.

10. Ulceration of both lips.

11. Irregularly-formed ulcer, and raised.

12. Extensive ulceration with everted margin.

13. Spreading ulcer.

14. Large and prominent ulcer.

15-17. Specific syphilitic ulceration.

18. Deep-seated ulceration, with hypertrophy of the anterior lip.

19. Deep-seated ulceration, exposed by bivalve speculum.

20. Ulcer beginning to heal, anterior lip.

21. Ulcer healing, more advanced.

22. Ulcer healing, still more advanced.

23. Ulcer healing, with irregular margin.

24. Healing of the anterior lip.

25. Healing of the posterior lip.

26. Surface healed, with the anterior lip congested, &c.

27. Surface healed.

28. Inflammation of the cervix, and ulceration of os.

29. Cervix and os tincæ often met with in sterile females.

Case II.

POLYPI OF THE UTERUS.

30-1. Small polypi attached to the external surface of the cervix.

32. Intrauterine polypus.

33. Small pendulous polypus attached to the external surface of the cervix.

34. Small pendulous polypus protruding at the os.

35-6. Extrauterine polypi.

37. Polypus attached to the os tincæ.

38. Small cystic polypi on the walls of the cervix.

39. Polypus attached to the side of the uterus.

40. Small pediculated polypi of the cervix uteri; disease of the body of the uterus coexisting.

41. Polypus attached to the fundus uteri.